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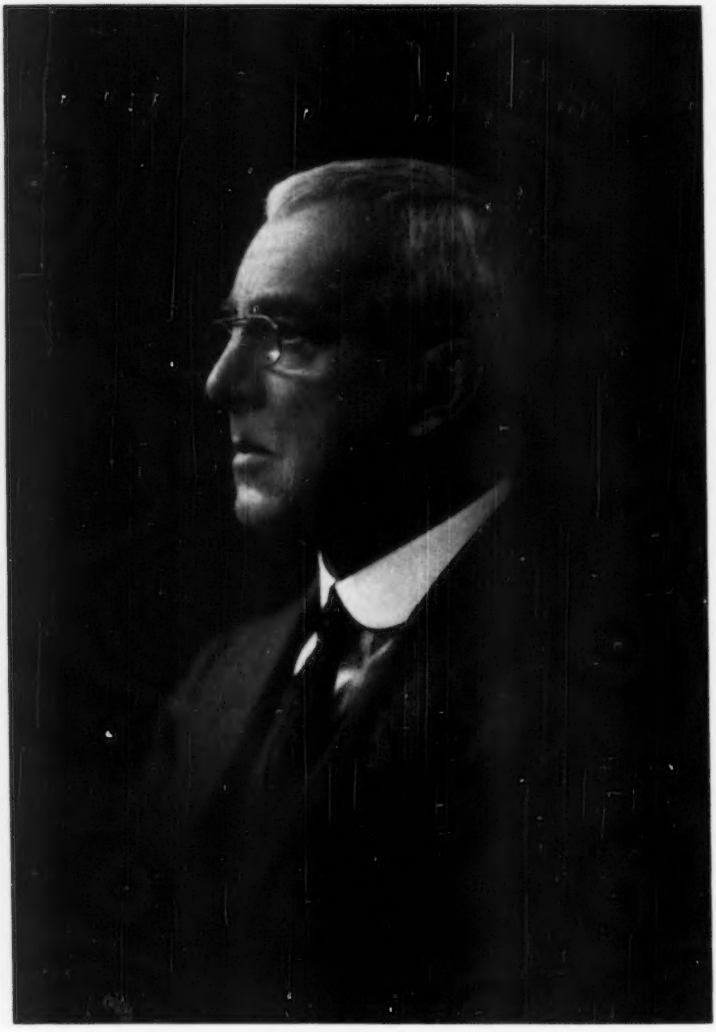

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R. H. Rastall

GEOLOGICAL MAGAZINE

VOL. LXXXVII. No. 2. MARCH–APRIL, 1950

OBITUARY

R. H. Rastall, 1871–1950

ROBERT HERON RASTALL, M.A., Sc.D., M.Inst.M.M., Fellow of Christ's College, Cambridge, died at his home near Whitby on 3rd February, 1950, aged 78. His name first appears as Editor of the *Geological Magazine* on the title page of the 1919 volume, and he overlapped with Dr. Woodward for three years; his death severs the last direct connection with the original editorship of this journal.

Born near Whitby in November, 1871, he was the elder son of the late Mr. H. A. H. Rastall, J.P., of Turnerdale Hall, Ruswarp, and he claimed to be descended from John Rastell, nephew of Sir Thomas More. He was educated privately, and later studied agriculture with the object of the better managing the family estates around Grosmont and Ruswarp in Eskdale. For a time he taught at Tamworth Agricultural College; but geology had fascinated him as a boy (when, as he recalled, " Martin Simpson was a familiar figure of every day life and in nearly every Whitby home there were ' snakestones ' "), and to geology at Cambridge he later returned. During the course of his professional career, his interests ranged over a remarkably wide field, but appropriately his first and his last papers were concerned with the geology of his native county.

His decision to devote himself to geology was taken relatively late in life, and he was nearly thirty years old when in 1899 he came to Cambridge to obtain a double first in the Natural Sciences Tripos (Part I in 1902, and Part II, Geology, in 1903). His Supervisor recalls him as one of the earliest undergraduate car owners, and motoring remained one of his pleasures to the last. In 1903 he was awarded the Harkness Scholarship, and in his early post-graduate period in company with others of his science year he engaged in part-time tutorial work at one of the London Civil Service coaching establishments. In 1906 he was elected to a Fellowship at Christ's College, which he held till 1913; he continued to reside in College, and was re-elected to a Supernumerary Fellowship in 1926. From 1905 to 1949 a steady stream of geological papers and books flowed from his pen, and till his retirement to Yorkshire in 1942, his association with Cambridge was only interrupted by the first World War, when his remarkable knowledge of languages was such that he rendered valuable

confidential service from 1915 to 1919 in the Censorship (at that time under the War Office). His ability was recognized at the University by his appointment, first as Demonstrator in Geology (in 1910) and later as Lecturer in Economic Geology (1919). He was awarded the Gold Medal of the Royal Geological Society of Cornwall in 1926, and the Lyell Medal of the Geological Society of London twenty years later.

Rastall's original geological work was so varied as to be difficult to summarize. In the field of igneous petrology and metamorphism his "Skiddaw Granite" (*Quart. Journ. Geol. Soc.*, 1910) is perhaps the best-known of several papers. As a sedimentary petrologist, he was one of the pioneers in directing attention to the significance of accessory minerals in sediments; more recently, his name is particularly associated with petrographic investigation of the sedimentary iron ores of the Jurassic. His papers on the tectonics and underground structure of southern and eastern England were admirable summaries and the best interpretations of knowledge in this field up to the recent phase of regional geophysical exploration. In stratigraphy, he was the first to present plausible evidence for the Pre-Cambrian age of the Ingletonian, and he has made notable contributions to our knowledge of the Middle Jurassic rocks of Yorkshire. In the economic field his main work was concerned with the genesis of tin and tungsten ores. It is, however, probably through his numerous textbooks that his influence has been most widely felt. Generations of students, far beyond the limits of the Cambridge School, have been introduced to geology through "Lake and Rastall", now in its fifth edition, and almost a household word. Hatch and Rastall's *Petrology of Sedimentary Rocks* ran rapidly through two editions, and was again revised shortly before the last war by Dr. Black. His *Agricultural Geology* combined his early interests with his later experience in a standard work of its day; and his *Geology of Metalliferous Deposits* and *Physico-Chemical Geology*, though never so popular as the others, were stimulating propaganda at a time when in this country economic geology was receiving but scant attention. Between 1906 and 1913 he travelled on the Continent, visiting type petrological localities; in 1910 he visited South Africa; and in 1927 he went at the invitation of the Federated Malay States Government to arbitrate in a dispute concerning the structure of the Kinta Tin Field.

Rastall's services to the *Geological Magazine* covered a period of more than thirty years. He had begun to assist Dr. Woodward in 1916, and in 1919 he assumed joint editorship, taking over full responsibility on Woodward's death in 1921. About this time the *Magazine* had been passing through one of its recurrent crises, and had in 1918 narrowly escaped extinction. The enthusiasm which Rastall brought

to it in the early post-war years, with the financial support of many generous subscribers, and from time to time as occasion required of the Editor himself, carried it safely past the critical period ; marked by the advent of " whole series " numbers and the disappearance of the tiresome " decades ", the *Magazine* emerged under Rastall's charge to a further phase of useful existence. For a while, he had to contend with a difficulty almost incredible to-day—shortage of copy—and upon the Editor devolved the duty of hack-writing to fill the standard number of pages. Many notes and reviews in those early years had no other object than this, but in all he wrote fluently and with clarity. He strove to introduce consistency and improve the general standard of presentation of papers, and finally published his advice in an article " On the Preparation of Geological Manuscripts " (*Geol. Mag.*, 1933). His own wide interests in geology, his good sense, and his broad-mindedness made him an ideal editor ; even his distaste for palaeontology was a tolerant aversion, and he granted " The language of palaeontology is a jargon, but at any rate it is a consistent jargon ". To the last, he upheld the duty of the *Geological Magazine* to afford publicity to unorthodox views, a traditional ideal which he inherited from his predecessor and instilled into his successor.

Rastall belonged to that once prevalent class of bachelor don whose College was his home ; and his conservative nature did not easily adapt itself to the changing social conditions that followed the first war, and were accelerated by the second. He had a reserved manner to which shyness and increasing deafness both contributed, but he hid beneath a cool and at times almost frigid exterior a warm-hearted and affectionate nature known to but few. In congenial company, he revealed himself a gifted conversationalist, admired and appreciated by such exacting critics as A. E. Housman and Ernest Harrison. Slow and deliberate in speech, his everyday conversation was characterized by a caustic humour, as when he solemnly warned his successor, " An author never corrects his proofs ; he merely alters them," or explained the basis of Geological Society awards : " When you have filled in a sufficient number of six-inch maps of Wales, they give you a medal." Truly he did receive the Lyell Medal in 1946 without this preliminary accomplishment, but success was more of a stranger to him than disappointment, and even his life at Cambridge was not unclouded by misunderstandings.

As an undergraduate, he became converted to the Roman Catholic faith, and from then onwards he was zealously associated with Catholic activities in the University. He was President of the Cambridge University Catholic Association when that body established Fisher House as the chaplaincy to the Catholic undergraduates in 1924, and for nearly twenty years after, so that recent generations of Catholic

students who thus came into contact with him socially will perhaps recall him more vividly than the geological students, for he gave up active teaching in 1934.

The last few years of his life were passed in lonely retirement on his family estate in Eskdale, after ill-health and the strain of the war years made it advisable for him to leave Cambridge. His aloof but courtly figure had already ceased to be familiar, but will long be remembered with affection by his friends and colleagues.

(In the compilation of this note, I have enjoyed the help of many who knew or worked with Dr. Rastall ; in particular I should like to thank the Rev. A. N. Gilbey, of Fisher House, Dr. W. L. Edge, and Professor W. G. Fearnsides, F.R.S.—O. M. B. B.)

The Study of Ammonites in Thin, Median Sections

By L. F. Spath [1]

Abstract

During the preparation of the second part of the Catalogue of Ammonoidea of the Trias, several hundred thin median sections of ammonites were prepared and examined. Structural features of such sections are shown to have helped in the interpretation of *Didymites*, *Lobites*, and *Paraganides*, and some reference is made to characters of other genera, mainly Trachyceratida.

POLISHED sections of ammonites have always been popular. They were figured by the early iconographers as well as by more recent authors and they are still sought by the visitor to famous fossil localities like Lyme Regis. They are useful for showing in a general way the internal structure of the ammonite shell as, for example, the magnificent "longitudinal section of *Ammonites obtusus*" figured by Buckland over a hundred years ago. Few of the sections, however, were truly median, and the inner whorls were often badly or wrongly restored and generally considered too small to be of any importance.

Hyatt, in his *Embryology* (1872), was the first to extend the principle of examining the internal characters of ammonites and to prepare median sections for study under the microscope. He made mistakes in interpretation and put forward startling theories which discredited his work in Europe. Moreover, Hyatt did not follow up his investigation of the internal features of the Ammonoidea; and in his renowned *Genesis of the Arietidae* (1889) he stated that the characteristics of the embryos and of the earliest stages did not yet seem sufficiently well known to be used for the classification of the Ammonoidea. These, according to him, were not divisible into two "grand divisions" but into six sub-orders; and in the recognition of these emphasis was laid on the external characters, mainly the suture-line.

Meanwhile Branco (1879–1880) published his famous researches on the internal features of ammonites and goniatites and these held the field for many years and influenced the work of all students of cephalopods. Branco had many followers in his purely ontogenetic work and the development of a number of individual ammonites became known; but with the exception of the brilliant researches of F. Grandjean (1910) few observations were made on the less obvious features of the inner whorls of ammonites and goniatites in general. It is not only that the preparation of median sections requires patience and technical skill, apart from almost unlimited material, in view of the many failures, due to a variety of factors. These range from the vagaries of mineralization to the wandering siphuncle which rarely lies in one

[1] Published by permission of the Trustees of the British Museum.

plane ; moreover, the original of a finished thin section cannot be redeemed for examination by ordinary methods and the attendant risk precludes from sectioning all type-specimens and most of the rarer forms—in fact, all examples too precious to be sacrificed. One investigator (Böhmers, 1936) found that in his collection of about a hundred preparations of Permian ammonites there were only four that showed a small portion of the siphuncular sheaths, while none of the sections retained the coecum (the bulb-like beginning of the siphuncle) or its supporting membrane.

Steady advance, nevertheless, is being made in this line of research and we now know the internal structure of a large number of goniatites, Permian and Triassic ammonites, while comparatively little has been done on Jurassic and Cretaceous forms since I discussed some of them in 1933. During the preparation of the second part of the Catalogue of the Ammonoidea of the Trias (about to be published by the Trustees of the British Museum), however, the writer had occasion to prepare and examine thin sections of most of the commoner forms of that period. For while some (Miller and Unklesbay, 1943) hold that the siphuncular structures (the most obvious features in median sections) are not so valuable, in taxonomy, as the (external) suture-lines, others (v. Voorthuysen, 1940) maintain that the internal characters are of the highest importance for determining the inter-relations of the different ammonite families. The evidence of the median sections, therefore, had to be taken into account, even though I myself had previously (1933, 1936) pointed out the vagaries of the siphuncle in the early stages and doubted its value for phylogenetic purposes.

It is not now intended to give more than a brief summary of some of the results of examining several hundred sections of Triassic ammonites. But before doing so I may direct the attention of the reader to some Liassic ammonites, described and figured by v. Voorthuysen, because they seemed to offer an excellent opportunity of testing the usefulness of median sections for purposes of identification of genera or families. It may occur to some that my former doubts as to the systematic value of the characters of the innermost whorls of ammonites were not unfounded.

The Liassic ammonites in question were stated to be "*Aegoceras* sp.", and they were said to come from a black limestone from Gloucestershire, scarcely auspicious representatives of the host of Jurassic ammonites. Van Voorthuysen, however, considered his own four sections to agree with a section of *Ammonites planicosta* (Sowerby) figured by Branco, and comparison was therefore made first of all with about a dozen sections of *Promicroceras* (*planicosta*-group) in the collection. It became clear that of all the characters tabulated by v. Voorthuysen (size of the protoconch, increase in whorl-height,

position of the initial constriction, spacing of the septa, thickness of the siphuncle, its position, direction of the siphonal funnels) only the first two were against identification of the doubtful "*Aegoceras*" with *Promicroceras*, the protoconch being only about ·35 mm. in the latter, instead of about ·5 mm., and the whorls increasing in the proportion of about ·30 : ·50 : ·75 in *Promicroceras*, instead of about ·35 : ·75 : 1·30.

Now the size of the protoconch may be a useful generic character in some Triassic forms, as mentioned below; unfortunately, it requires a perfect, median section, with the innermost whorl intact. In the Jurassic, too few observations have so far been made; and as regards the "*Aegoceras*" in question, it suffices to mention that not only in *Androgynoceras* (of which "*Aegoceras*" is a synonym) but in many other genera formerly included in "*Aegoceras*" (from *Schlotheimia* below to *Oistoceras* at the top of the Lower Lias) the size of the protoconch does not differ materially from that of *Promicroceras*. So this first character is of little use in our attempt to identify the doubtful "*Aegoceras*". The second character, namely the increase in the rate of growth (of the spiral or whorl-height) agrees with that of *Androgynoceras*. But in the adult there is often scarcely specific value attached to this increase in coiling; and in *Androgynoceras* both slowly increasing (evolute) and more quickly coiled (involute) types occur, with many intermediate forms in between. Nevertheless, in the very young of Triassic ammonites the increase in whorl-height has been found to be a useful character; and the same applies to the spacing of the septa in some groups, although on the whole this is probably the most unstable of all the characters.

Van Voorthuysen's "*Aegoceras*" has 16 and 18 septa in the first two whorls, which compares well with the numbers (14 and 15) in a typical section of *Promicroceras* before me, or with 16 and 20 in Branco's section, already referred to, and 14 and 17 in a section figured by Hyatt (1872).[1] In another excellent *Promicroceras* section, however, there are only 7 and 8 septa in the first two whorls, whereas in a typical section of *Androgynoceras* the numbers are 11 and 7. Not to overload this account with negative evidence from still more sections, I may say that the number of septa will not enable us to identify the "*Aegoceras*", any more than the position of the initial constriction which in any case is always near the end of the first whorl and which is at a different angle in each of the four sections examined by v. Voorthuysen.

The siphuncle in *Androgynoceras* may become external after only 1½ whorls, instead of at the end of the second whorl, as in *Promicroceras* or the doubtful "*Aegoceras*", but there is variability also in this character, for in one *Promicroceras*-section the siphuncle remains

[1] The locality "Wiltshire" may be in error for Somerset (Marston).

away from the periphery for 2¾ whorls. The width of the siphuncle is more or less constant in all the genera mentioned, but the character of the siphonal funnels of the first whorl could not be observed in any which leaves the "*Aegoceras*" as doubtful as ever. A glance by an expert at the actual specimens, before sectioning, might have settled their identity.

In the case of the far less well-preserved Triassic material it would be surprising if results were entirely satisfactory, but it will be advisable to let the evidence speak for itself. In any case, all the observations made (such as on structure and position of the siphuncle, the protoconch, the septation, the coiling, etc.) have been recorded in the Catalogue, in case future investigators with access to the many genera necessarily omitted from the present account are in a better position to evaluate the seemingly contradictory evidence. Thus, a first difficulty was encountered in a sharp-edged genus (one of the Middle Triassic family Hungaritidae) in which, as expected, the siphuncle was external from the start but only in six sections, and not in one. In this isolated section the siphuncle was still away from the venter (i.e. the periphery) at the end of the septate stage. Now the possibility of an error in the identification of that particular specimen cannot be ruled out ; but it could only have been an example of a closely allied genus which happens to occur in the same rock. An examination of five specimens of that form again showed the siphuncle to be external from the protoconch onwards though, of course, in the average imperfect section the siphonal funnels (which are much more frequently preserved than the siphuncular tube) are never seen along the entire spiral even when there are no complications through secondary mineralization.

The isolated section just mentioned may represent an individual aberration or a pathological specimen. If so, it need cause no misgiving, for an anomalous case can always be checked by additional sections. At the same time, however, it could be held to indicate an inherent instability of the position of the siphuncle, not only found in goniatites but liable to reappear in any later stock. In another sharp-backed genus, the Lower Triassic *Owenites*, Hyatt and Smith, a certain Timor species was found by Böhmers to have at first a central and then a permanently centro-ventran siphuncle ; but on examination of an example of the original Californian type-species in the Collection, it was discovered to have an exactly similar, non-external siphuncle to the end. The position of the siphuncle in *Owenites*, therefore, is obviously a generic character. Protection of the siphuncle has been claimed to be a function of the keel or sharpened periphery. In one genus (the Upper Cretaceous *Hoplitoides*) the original asymmetry of the siphuncle is lost when the venter becomes acute. On the other hand,

in another oxynote, i.e. acute-ventered Cretaceous genus (*Platylenticeras*) the siphuncle is known to have been permanently displaced to one side and this has been explained as an adaptation to a benthonic mode of life (like flatfish). The position of the siphuncle in the sharp-backed forms above mentioned cannot have been due to such asymmetry because it was too far below the venter.

Moreover, in round-backed genera also, e.g. in *Didymites*, *Lobites*, *Paraganides*, etc., the siphuncle is not yet external at the end of the chambered stage and the same applies to certain Trachyceratids in which the periphery has a median groove, like the Californian *Traskites*. The genus *Didymites* is of late Triassic age and though it has also been grouped with Arcestids of somewhat similar appearance it is almost certainly a member of the Tropitida. It is connected with them not only by external characters and transitions like *Indonesites* Welter, but it has the same thick siphuncle, septal funnels, and other internal characters of that family. The Tropitidae, however, appear in great numbers in the Carnian, at the base of the Upper Triassic, and their siphuncle is internal or central for only a short stage and becomes external after only about 2½ to 3 whorls, i.e. at a diameter of some 5 or 6 mm. *Didymites* may then be looked upon as a group in which the position of the siphuncle away from the venter is retained much longer than in the ancestral Tropitidae.

In *Lobites* the internal evidence had to be pieced together from ten sections because the inner whorls were partly or entirely replaced by crystalline calcite. Nevertheless the wide siphuncle could be seen to be subventral ; and in the largest section, the last septum, at 21 mm. diameter, well shows the siphonal funnel which is definitely away from the periphery, though less so than in *Didymites*, *Paraganides*, or *Traskites*. In spite of its wide siphuncle and small protoconch I am not including *Lobites* in the Tropitida ; for it not only occurred already in the Ladinian (Middle Triassic) but it specialized in its own way in its external characters, even in the suture-line. Hyatt therefore quite rightly created for *Lobites* a separate super-family, and it can well be kept distinct from other stocks, including the Arcestida with which *Lobites* had been grouped by most authors. These have a large protoconch and a slender, thin siphuncle, at least on the outer whorls and in the later, Upper Triassic species. In the early, Middle Triassic *Proarcestes* the siphuncle also is thicker and external already at the end of the first whorl ; in the later true *Arcestes* it becomes external only at the end of the second whorl. But they are both characterized by their polygyral coiling and close septation, so that at 18 mm. diameter a section of *Arcestes* shows 23 septa to the whorl, one of *Lobites* at same diameter only 9.

Paraganides, Hyatt and Smith, is another genus whose interpretation

was facilitated by median sections. The degenerate suture-line completely baffled its authors who grouped *Paraganides* with the Nannitinae. Although I consider that this genus can be recognized as a Tropitid (of the family Haloritidae) by its general resemblance to *Anatomites*, yet its thick, centro-ventran siphuncle is conspicuous and was in fact described (Spath, 1933) as a *Tropites*-siphuncle. As Böhmers (1936) stated, it is internal at first, then central and it becomes centro-ventran (a third of the whorl-height away from the venter) in the fifth whorl. This is the end of the septate stage, at 6 mm. diameter, so that, like *Didymites*, it retained throughout its ontogeny the internal (i.e. non-ventral) siphuncle of the inner whorls of *Tropites*. The equally degenerate and related *Tardeceras* of the same family also has the small protoconch and thick siphuncle of the Tropitida but the position of the latter is subventral, as in *Lobites*, rather than centro-ventran, as in *Paraganides*. *Tardeceras*, which is not the same as *Metasibirites*, as J. P. Smith (1927) thought, is characterized in all six sections by its unusually thick test.

Thickness of the test as well as of the septa may eventually prove of generic importance ; so far the writer has not found this character helpful except in isolated cases like that of *Tardeceras*, just mentioned, or of *Norites* which seems to have unusually thick septa. It should be recorded, however, that the peculiar structure of the test in thin sections revealed an unexpected affinity between *Hannaoceras* (formerly " *Polycyclus* ") and *Leconteiceras* (*olim* " *Leconteia* "). A resemblance had been admitted, but J. P. Smith (1927) pointed out that the young of *Leconteiceras* were not like those of the Ceratitidae but subglobose like those of *Sagenites*. There is no resemblance in thin sections, for *Sagenites* is quickly coiled, having about 3½ whorls at 12 mm. diameter, whereas *Leconteiceras* is polygyral, with 5½ whorls at the same diameter. Nevertheless the latter genus was assigned to the family Haloritidae of the Tropitoidea. In median sections both exhibit a siphuncle that is not quite external at the end of the septate stage and a small protoconch, so that they could equally well be Tropitida or Trachyceratida. *Hannaoceras*, however, is of almost world-wide distribution whereas *Leconteiceras* is confined to California and is possibly merely an extreme development of *Hannaoceras*. If the latter is correctly included in the Trachyceratida (family Choristoceratidae) then *Leconteiceras* may have to be removed from the Haloritidae. It should be added that the peculiar folding of the middle layer of the test which was first noticed in a Californian form of *Hannaoceras henseli* (Oppel) and revealed its close affinity with *Leconteiceras*, is equally developed in a Hallstatt example of Oppel's species. Unfortunately it is uncertain whether the siphuncle in the latter is subventral as in the Californian specimen.

Traskites, already mentioned as one of the genera with internal (centro-ventran) siphuncle to the end is one of the Clionitidae or evolute Trachyceratida; but even in these the sections show the more rapid increase in coiling which all Trachyceratida inherited from their Ceratitid ancestors with external siphuncle. The tendency to delay its passage to the exterior was found also in species of *Trachyceras*, *Nevadites*, *Klipsteinia*, and *Drepanites*, in all of which the siphuncle was not external at the end of the septate stage.

In some Trachyceratids it may be difficult to decide whether the siphuncle is quite external or not, especially as truly median sections are rare ; and as the examples sectioned were comparatively small, I had cut and polished a large specimen of *Trachyceras aonoides* (diameter = 90 mm.) to compare with that figured by Mojsisovics (1893). The siphonal funnels do seem to become external after about 15 mm., but the outer half of the funnel remains conspicuous to the end of the septate stage, at just over 50 mm. diameter, and the siphuncle does not touch the shell-wall as it does in Ceratitids. There is no doubt, however, of the position of the siphuncle in the case of *Traskites*, because the siphonal funnels can be seen to the last septum in two sections, at 8 and 13 mm. diameter respectively, and they are well away from the venter. The protoconch (·3 mm.) is smaller than in some other Trachyceratids, but in *Drepanites* it is not much larger and in the Ceratitids (e.g. *Gymnotoceras*) it is the same (·35 mm.)

In *Arcestes*, on the other hand, the protoconch is ·5 mm. in diameter and in *Joannites* even ·6 mm. In case, however, too much importance be attached to these differences, I hasten to add that in *Cladiscites* (of the same super-family Arcestida) the protoconch is only ·42 mm. in diameter, as it is, at the same size, in an entirely unrelated species of *Discophyllites*, whereas in a form of *Diphyllites* it may be as small as ·29 mm. Since these two forms belong to the only ammonite family that transgressed the fatal Triassic-Liassic border, it would be particularly interesting to have more information on the Jurassic and Cretaceous members of the Phylloceratida, the most stable of all ammonite stocks.

Without going into detail concerning the remaining Triassic families, I may mention that useful evidence has been obtained for example in Ptychitidae and Gymnitidae, as recorded in the Catalogue. The investigation of Pinacoceratidae, however, was less successful, partly on account of the extreme slenderness of the shells, partly because of lack of duplicate material. In this connection I should like to acknowledge with gratitude the expert assistance I received from the technical staff of the Geological Department of the British Museum, especially from Miss P. Hammond.

Enough has been said to show how the evidence of the internal

characters is now being utilized in the interpretation of ammonites. There is as yet plenty of scope for the study of ammonites in thin sections, and in time, no doubt, this will be extended to ever more features so far neglected. It may be for future generations to evaluate all the evidence, but in view of the essential uniformity of the Ammonoidea as a whole and the difficulties of classifying a group in such a state of flux, it seems to me that every additional source of information is to be welcomed.

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Contact Metamorphism at Maddocks Hill near Wellington, Shropshire

By RANDOLPH W. CHAPMAN

ABSTRACT

A large basic sill intrudes the Shineton shale (Cambrian) at Maddocks Hill, east of the Wrekin, near Wellington, Shropshire. An old quarry exposes the contact and reveals that the shale close by has been recrystallized to a hard, compact hornfels, the intensity of alteration decreasing away from the contact. Thin sections show that the metamorphic effect was solely thermal, the rock having been completely recrystallized, and biotite having been generated under conditions of the amphibolite facies. The metamorphism is compared with that brought about by other basic hypabyssal intrusions.

GENERAL STATEMENT

THE late Ordovician (?) igneous intrusion cutting the Shineton shale (Cambrian) at Maddocks Hill, east of the Wrekin, near Wellington, Shropshire, has been described several times in the literature. Over 100 years ago it was mentioned briefly by Prestwich (1840) and its position indicated on his map. It was mentioned again, many years later, by Lapworth and Watts (1894), and then more recently by Whitehead, *et al.* (1928), and Pocock and Whitehead (1938 and 1948).

The Maddocks Hill intrusion is lenticular and generally sill-like, although perhaps locally cross-cutting. According to Pocock and Whitehead (1938, p. 73) it trends north-east, parallel to the strike of the enclosing Shineton shale, for a distance of 900 yards. Its maximum width is about 90 yards, but near its south-west end, in the old Maddocks Hill quarry, it is only some 35 yards wide. In this quarry one contact of the intrusion is well exposed and is seen to dip 80° S.E., parallel to the bedding of the shale.

Several writers have recorded the fact that the intrusion hardened and altered considerably the adjacent shale and, according to Pocock and Whitehead (1938, p. 68), in the quarry mentioned above this alteration extended to a distance of 25 yards from the contact. As far as the present writer is aware, however, the contact metamorphism has never been fully described, and its character remains unknown.

In September, 1948, the writer made a brief visit to the Maddocks Hill quarry and studied rather cursorily the sill itself and the contact zone in the adjacent shale. Specimens were collected and studied microscopically, with a view to determining the character of the metamorphism. The information gained, although admittedly scanty, is here placed on record in the hope of adding something to our rather meagre knowledge of the contact-metamorphic effects of hypabyssal intrusions.

Intrusive Rock

The Maddocks Hill intrusion was called a camptonite by Watts (1925, p. 360), but his application of this term is somewhat questionable. Hand specimens of the rock are fine-grained and massive, and they break with a rude conchoidal fracture. Fresh surfaces are mottled, pink feldspar crystals being intergrown with anhedral masses of greenish black chlorite altered from augite. The rock is quite equigranular and without phenocrysts.

Pocock and Whitehead (1938, pp. 73–4) described the rock as follows: "Under the microscope the rock . . . is seen to consist of a plexus of crystals of turbid albite-oligoclase edged with alkali-felspar, sub-ophitic to hypidiomorphic augite, a few serpentine pseudomorphs after olivine, some biotite and brown pyrogenetic hornblende. The interstices between the felspars are variously filled with chlorite and secondary zeolitic minerals, while alkali-felspar can be detected in some instances. Apatite is a common accessory and titano-magnetite occasionally surrounded by granular sphene occurs sparingly throughout in moderately large crystals. Occasionally . . . the rock exhibits a definite ocellar structure, the ocelli being occupied by small idiomorphic crystals of augite in an indefinite base."

That part of the intrusion exposed in the quarry differs mineralogically from the rock described in general terms above. It has typical ophitic texture with laths of andesine oriented at random and the interspaces filled with augite, chlorite (from augite), and ilmenite. Accessory minerals include potash feldspar (?), apatite, red-brown biotite, titanite, and glass. The grain size averages about 0·08 mm. The rock is badly altered, probably hydrothermally, most of the augite having been converted to chlorite, with only isolated relics remaining. Andesine crystals, particularly their cores, are altered to sericite and some kaolinite and calcite. Their borders may actually be potash feldspar, but this is uncertain, since they are deeply clouded with specks of haematite. The plagioclase is more calcic than that described by Pocock and Whitehead above, and hornblende, serpentine, and zeolites are absent.

Slightly Metamorphosed Shale

Even casual inspection of the shale shows that it is more intensely altered adjacent to the intrusion than at a distance. No specimen of completely unaltered shale was collected by the writer, but one that appeared to be only slightly affected, i.e. mainly hardened, was collected from a point 8 to 10 feet from the north-west contact of the intrusion. This rock is exceedingly fine-grained, light greenish grey, and has a high degree of fissility. It can be barely scratched with the fingernail.

The microscope reveals a uniform grain size, most crystals having

a diameter of about 0·013 mm. Fineness of grain makes identification uncertain, but the chief constituents appear to be sericite and biotite, which compose 75 per cent. Sericite forms clean, sharp crystals, with planar arrangement. Biotite, on the other hand, is less clearly defined, and makes up somewhat irregular, fibrous, yellow-brown aggregates, with a cruder planar orientation. These oriented micas produce the fissility. Scattered grains of quartz and feldspar together form perhaps 20 per cent of the component minerals. The balance of the rock (i.e. 5 per cent) consists of granular rutile and magnetite and small specks of what appears to be carbonaceous material. Slight recrystallization is evident in this specimen, the principal change having been the formation of biotite at the expense of chlorite, sericite, magnetite, and rutile of the parent shale.

Strongly Metamorphosed Shale

A specimen of rather strongly metamorphosed shale was collected 2 inches from the south-east contact of the intrusion. This rock, like the one just described, is light greenish grey. However, it is harder, appears more dense and compact, has lost most of its fissility, and has developed a conchoidal fracture.

In thin section a number of differences are evident. The rock is finer grained than the slightly metamorphosed shale, the average grain size here being about 0·010 mm. This change has been chiefly in the micas, quartz and feldspar retaining their original size. Moreover, the micas have lost their good orientation; they are now randomly arranged and many, especially of the biotites, are aggregated into clumps. Considerable recrystallization is evident and the groundmass appears hornfelsic. The differences between the slightly and strongly metamorphosed shales seem to be mainly textural, the latter rock having a mineral composition similar to that of the slightly metamorphosed shale farther from the contact.

Conclusions

Judging from published descriptions and from evidence obtained in the quarry, the contact effect of the Maddocks Hill intrusion on the Shineton shale was solely thermal. Heat from the sill recrystallized the soft fissile shale, generated biotite, and developed a hard, compact, hornfelsic rock with conchoidal fracture. Alteration was most intense immediately adjacent to the sill boundaries and decreased outward from them. The mineral assemblage of the altered shale near the intrusion is that of the amphibolite facies, indicating that temperature and pressure were moderate during metamorphism.

There is no evidence of hydrothermal alteration of the shale, nor does it appear that elements were brought in or removed. The

intrusion, however, was affected by hydrothermal solutions that probably originated within itself and rose from depth. It appears probable that these were unable to move laterally across the bedding planes of the impervious shale.

The contact metamorphism brought about by the Maddocks Hill sill may be compared with alteration caused by other basic hypabyssal intrusions. It is similar in nature and intensity to that of the great Whin sill in Northumberland and Cumberland. Hutchings' (1898) description suggests that the Whin sill altered its country rock under moderate temperature and pressure. There is little evidence of the work of hydrothermal solutions, although soda was added to some of the sediments. At least some of the altered sediments adjacent to the Palisades sill (Lewis, 1907) in New Jersey and New York, belong to the pyroxene hornfels facies, so that the temperature to which they were raised by this huge intrusion must have been much higher than that of the Shineton shale. Recent studies by the present writer (in press) of the contact effects of a large Triassic diabase dyke at Safe Harbor, Pennsylvania, show that temperature-pressure conditions of metamorphism were similar to those adjacent to the Palisades sill. However, late hydrothermal alteration was intense, perhaps partly because the intrusion is cross-cutting and solutions were free to move outward along bedding and schistosity planes.

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Metamorphism of the Trias in the Alps

By R. Perrin and M. Roubault

Abstract

In a previous number of the *Geological Magazine* Professor O. T. Jones questioned the existence of the metamorphism of the Trias in the autochthonous Alps. The authors give a reply stating accurately their observations. They also recall similar observations made by other authors.

The assemblage of facts cited enables us to assert that this metamorphism exists. The problem is very complex indeed ; but the fact that this metamorphism seems to be in contradiction to the classical theories about alpine orogenesis does not allow one to deny its reality. The authors maintain that the solution of the problem must be looked for in the new ideas on " fronts " of metamorphism.

"IT seems I was wrong in thinking as I did then that folly could no higher go, for here we have a case of investigators claiming to prove from the examination of some thin slices of rocks that one of the best established unconformities in Europe is nothing but a delusion, of which a great number of geologists whose names are known wherever geology is taught have one and all become the victims."

Such is the conclusion of an article by Professor O. T. Jones (1948), relating to a paper of ours (1941) dealing with the metamorphism of Triassic rocks in the Alps and the recognition of a front of metamorphism. This paper had been reviewed by Doris L. Reynolds, in the *Geological Magazine* (1947).[1]

On reading such an authoritarian judgment, we seemed to hear again the sentence passed by other " authorities " on our idea, expressed some fourteen years ago, that metamorphism and granitization depend on diffusion in the solid state. Yet to-day, many authors, even including some of the highest authorities on magmatics, agree that some granites were formed by metamorphism, without passing through a liquid condition. The recent discussions at the International Geological Congress in London were symptomatic in this respect. We should not, therefore, be seriously troubled by this new condemnation, were it not that it is stated in terms that may lead to a misunderstanding of our work, and—much more serious—provoke doubt as to the correctness of our observations both in the field and under the microscope. Moreover, a reply from us is the more necessary, because H. H. Read, in citing this same article of ours, writes " if these workers are correct ", and declares himself " unable to agree ".

[1] Since the publication of O. T. Jones' paper, a letter by E. B. Bailey, dealing with the same subject, has appeared (*Geol. Mag.*, 1949, p. 73). In this note Bailey confines himself to a discussion of the effects of weathering on older rocks, whilst our paper was concerned only with the results of a more recent metamorphism.

From reading Prof. Jones's remarks, it could be concluded that we have committed a grave error, by mistaking for the Trias some " peculiar rocks " which are nothing other than " vieux cristallin décomposé ". The reader might thus be led to question whether the observations that led us to the idea that granites are formed in the solid state are correct, or whether, for example, we did not confuse granite with limestone, or feldspar with quartz ! Fortunately, in so far as granite is concerned, we have the witness of H. H. Read himself, who reaches the same conclusions.

We regret that our reply to Prof. Jones cannot be other than condensed, for a re-discussion of the whole problem would become too lengthy. There are, however, four main questions to be answered :—

(1) Are the rocks that we observed really Trias ? If so, are they metamorphic rocks ?

(2) Is there any other metamorphic Trias in the autochthonous massifs of the French Alps ? If so, what is its origin ?

(3) Is a " front " of regional metamorphism a possibility ?

(4) What is to be thought about the Hercynian unconformity in the Autochthonous Alps ?

We shall deal in turn with these four questions.

(1) *Are the rocks that we observed really Trias ? If so, are they metamorphic rocks ?*

Our own observations were made in Savoy, at the Mollières d'Ugine, at Feug, and on the floor of the dam at La Girotte, near Beaufort. They concern concordant beds of silicious conglomerate containing pebbles of rose quartz, quartzite, and dolomite. These beds underlie the Lias concordantly, and without exception they have been recognized as being of Triassic age by all the authors—Auguste Michel-Lévy, Duparcq, Ritter, M. Gignoux, L. Moret, *et aliter*—who have studied the region. They are shown, as such, on the geological maps of the area. There is no " peculiar rock ". At the Mollières d'Ugine and at La Girotte, in particular, the contact of the Trias with the crystallines is perfectly sharp. If, as Prof. Jones writes, in certain places the Triassic sea washed the weathered crystallines clean, then we must acknowledge that the washing was well done.

At La Girotte it was possible to examine the series centimetre by centimetre throughout a height of about 30 metres, thanks to the cores of borings made when the dam was constructed. The crystallines lying concordantly at the base of the series are perfectly fresh, without any trace of alteration or reddening. Within the space of a few metres there is a rapid but progressive passage from a rock that is identical with the prasinites of the internal zones of the Alps, through Trias, to completely unaltered Lias. Yet, within a bed of Triassic quartzite

situated, as is commonly the case in this area, beneath a bed of dolomite, we observed crystals of newly formed amphibole, several centimetres long. How can this mineral be supposed to have originated otherwise than by metamorphism ?

In the region of Ugine a crystalline schist, composed essentially of alternating layers of quartz and green sericite, abuts against Triassic conglomerate containing pebbles of pink quartzite. In many places the contact between the two rocks appears sharp to the naked eye ; at most, one observes a slight transition extending through a few centimetres. Yet, locally, the same crystalline schist encroaches upon the conglomerate and encloses pink quartz pebbles with dimensions of the order 1–2 cm. In addition, true veins of very fresh schist penetrate the conglomerate and surround quartz pebbles. We shall be grateful if Prof. Jones will give us his interpretation of this phenomenon. M. Lugeon writes—though without insistence—of the " régénération des débris des roches décomposées de l'ancienne pénéplaine ". It is a curious " regeneration " that is able to give rise to rocks and veins composed of alternating layers of quartz and mica similar to subjacent crystallines which have, up to the present, been considered as an indubitable product of regional metamorphism. How is a process of " regeneration " able to give rise to a rock identical with one formed by regional metamorphism ? [1]

At Feug, within a bedded rock that is absolutely identical, both megascopically and microscopically, with the Triassic conglomerate characterized by little rose quartz pebbles, we have found, close to zones in which there has been simple recrystallization of the quartz and sericitization, certain layers with a gneissic aspect, and others containing newly formed muscovite and tourmaline crystals. Does Prof. Jones think that here the Trias has been metamorphosed and, if not, how does he explain the genesis of the layers with a gneissic aspect, and the muscovite and tourmaline crystals ?

On the whole, the beds of Triassic dolomite in the area studied show no particular aspect of metamorphism, at least to the naked eye. Near

[1] To suggest that two such identical rocks, which pass the one into the other, have had totally different origins is to our minds—if the image employed may be excused—like supposing that some men have descended from the anthropoids and others from the rhinoceros. It is for the same reason—at the risk of being once again called " enthusiasts " by Read—that we refuse, unless we obtain absolute proof to the contrary, to think that rocks with such an individual structure as the granites have been formed some from a state of magmatic fusion and others by diffusion through solid rocks. Without proof we cannot agree that there are " granites and granites ". In our experience the granitic structure is characteristic of crystallization in the solid state concomitantly with chemical interchanges. We do not, however, deny the possibility that some granite may be formed by the direct recrystallization of solid rocks, such as lavas, although we have not personally observed any such example.

Roselend, however, one of us jointly with P. Bordet observed a bed of dolomite, with many large crystals of newly formed chlorite. We hope that here, again, Prof. Jones will agree that the dolomite is really Triassic, and will not call it " peculiar rock ".

(2) *Does metamorphic Trias exist in the Autochthonous Alps? If so, what is its origin?*

Prof. Jones, following M. Lugeon, places particular importance on the observations made in Switzerland, and especially on those made at the famous section of Scheidnössli. It is essential to reconsider this point, be it only to reveal the " objectivité " of those who deny our observations. Prof. Jones repeats, almost word for word, the description of Scheidnössli, as given by M. Lugeon in 1946, referring to W. Staub (1911), who studied the section in detail jointly with E. Argand and B. G. Escher. We should have been pleased if Prof. Jones, before writing his paper, had made himself acquainted with our short reply to M. Lugeon on this point (1947), and still more so, if he had taken the precaution of referring to the original sources, that is to say, to W. Staub's own paper containing his description and sections. He would then have found that to describe this section as Lugeon does is to introduce a simplification that may well lead the reader into error : that is, to describe lower Trias formed of alternating beds of dolomite, shale, and quartzite, as resting upon a peculiar arkose that passes downwards into gneiss and displays cleavage and puckering that have given rise to an alignment of the grains which *does not exist in the normal Trias*.

W. Staub describes the section at Scheidnössli, numbered from bottom to top, as follows :—

1. " gneiss à beaux plis allongés de biotite ;
2. " arkose à enclaves de dolomie, présentant les mêmes plis allongés de biotite ;
3. " un banc horizontal de dolomie de 40 cm d'épaisseur, riche en quartz ;
4. " un nouveau banc d'arkose à *plis de biotite ;*
5. " un nouveau banc de dolomie de 1 a 2m où s'insinuent des veines d'arkose, *toujours à plis de biotite ;*
6. " une alternance de lits de dolomie de moins en moins riche en quartz et d'*arkose de moins en moins riche en biotite ;*
7. " une couche de calcschistes sériciteux, etc. . . ."

Must we conclude from the writings of M. Lugeon and O. T. Jones that the rocks interstratified in regular beds between beds of dolomite are not Trias ? Indeed, this is hardly admissible, yet these beds have some " plis de biotite " similar to those of the subjacent gneiss. If Prof. Jones had read the article written by W. Staub, he would have

seen that the latter, though expressing the opinion that the arkose is probably composed of material disintegrated in situ, frankly writes [1] : " The origin of the folds is still unexplained, for the deformation cannot be a folding of formerly horizontal beds." Staub suggests as a working hypothesis that pressure regenerated the arkose into " gneiss-arkose ", a hypothesis invoking a mechanism which appears, judging from the literature, to have been applied only in the case of Scheidnössli. What subtle distinction is it possible to make between metamorphism and regeneration, both of which appear to imply alteration dependent on recrystallization ?

With regard to the inclusions, or " nodules " of dolomite, within the arkose situated beneath the Triassic dolomites, Prof. Jones writes : " Clearly the mineral matters of the nodules have been carried by descending waters." If he is correct, then the inclusions are post-Triassic. It is therefore of interest to notice that W. Staub records that the *folds of biotite within the arkose become deflected around the inclusions of dolomite*. These folds of biotite are thus, according to Jones himself, also post-Triassic ; they are, however, quite similar to folds of biotite within the subjacent gneiss.

What should one conclude from Staub's description ? We shall be grateful if Prof. Jones will give us a clear and scientific interpretation of the section of Scheidnössli. Obviously the problem is not solved by applying the name " peculiar rocks " to the beds of arkose interstratified with the dolomite. In 1947, after M. Lugeon's communication, we referred briefly to observations made by W. Staub, E. Argand, and B. G. Escher (which M. Lugeon qualifies as high-class observations), and we then indicated the problems they raise. No reply has been made. We hope that our English-speaking readers will at least realize that the facts are much less simple than the writings of our critics might lead them to suppose, and that certain rocks which are undoubtedly Triassic have undergone post-Triassic transformations. Most troublesome to explain is the fact that these transformations have led to the production of rocks that are closely similar to the neighbouring crystallines.

Apart from Scheidnössli, especially invoked against us by M. Lugeon and O. T. Jones, other facts have been observed in Switzerland that are equally difficult to explain. Geologists such as Baltzer, Albert Heim, K. Rohr (1926), who have made detailed investigations of the well-known " coins mésozoïques " enclosed within the crystallines of the

[1] " Die Arkose zeigt ein deutliches und sehr schön ausgeprägtes clivage, welches in dieser Stelle das aufgearbeitetes Material durchsetzt Diese Fältelung der Arkose zwischen den Dolomitbänken ist nur in dieser Stelle so ausgeprägt. *Ihre Entstellung ist da es sich nicht um Fältelungen von früher horizontalgelagerten Schischten handeln kann — noch unaufgeklärt.* Es ist dasselbe Clivage, welches wir schwach angedeutet schon in den Injections-gneissen der Interlage festgestellt haben."

Aar Massif, have several times observed the development in the " zwischenbildungen post-hercyniennes et mésozoïques, de lits de mica dont la direction prolonge rigoureusement celle des micas des gneiss ". The micas of the " zwischenbildungen " are certainly post-Triassic and probably Alpine ; what is the probability that in different places, micas of Alpine age and others of Hercynian age should have absolutely the same alignment, although formed at times so far apart ? What is to be concluded ? Albert Heim writes [1] : " The bold schistosity of the old crystallines beneath the sedimentary mantle in the said places is not a proof of Hercynian folding but is rather due to Tertiary Alpine metamorphism . . . Such appearances have led me formerly to attribute the greater part of the discordance between the old crystalline and the Mesozoic to Alpine dynamometamorphism, without Hercynian folding." Rohr writes [2] about the Schwartze Naht : " The schistosity of the gneiss, like that of the arkose, is due to the Tertiary orogenesis."

Thus we see the development of a hypothesis of Alpine dynamometamorphism not only of the Mesozoic, but also of the crystallines of the autochthonous massifs of the Alps. It is really difficult, if not impossible, to discern the part reserved to this " dynamometamorphism " by the different authors in the crystallines of the various massifs : Aar, St. Gothard, Aiguilles Rouges, Mont Blanc, Belledonne, Pelvoux, etc. It emerges, however, that the observed facts have compelled eminent authors to admit that there was a recrystallization of the crystallines during Alpine time, at least in certain places, without—in face of the concordance of the schistosity throughout the whole assemblage of the massifs—being able to define the limit of this recrystallization. When precise observations similarly oblige us to re-discuss the age of the crystallines " en leur état actuel " in other parts of the autochthonous French Alps, and at the same time to reconsider the origin of the unconformity where it exists, we surely hardly deserve to be charged with folly.

We could quote many other troublesome facts and pose other indiscreet questions relating to the official interpretations, but we must limit ourselves. We shall, however, at least protest energetically against the epithet " so-called " applied by Prof. Jones, after M. Lugeon, to the metamorphosed Trias in the Autochthonous Alps. Whether M. Lugeon or O. T. Jones wish it or not, metamorphosed

[1] " Die Steilschieferung des Altkristallin unter dem Sedimentmentel ist an solchen stellen kein Beweis für herzynische Auffaltung, sondern ist weit mehr tertiäralpine Dynamometamorphose, was so oft übersehen wird . . . Solche Erscheinungen hatten mich früher (Mechanismus der Gebirgsbildung') dazu geführt, die Diskordanz von Alterkrystallin und Mesozoikum grösstenteils alpine Dynamometamorphose ohne herzynische Faltung zuzuschreiben."

[2] " Die Arkose wie die Gneisschieferung verdanken ihre Entstehung dem tertiären Gebirgsdruck."

Trias there is in these massifs, and even metamorphosed Lias ; we have not the honour of being the innovators in this matter. A long time ago Duparcq and Ritter reported from the zone we have studied a metamorphism of the Carboniferous and of the Trias similar to that of " La Vanoise " ; a long time ago Swiss investigators recognized metamorphic Mesozoic within the synclines of the western and eastern parts of the Aar Massif ; a long time ago Paul Niggli reported a clear metamorphism of Triassic rocks with development of ottrelite, chloritoid, etc., and an incipient development of " schistes lustrés " from Liassic rocks within the syncline of Urseren, between Aar and Gothard. Up to the present, however, Swiss authorities have ascribed this metamorphism to an Alpine dynamometamorphism. Our personal contribution is essentially the description of some specific examples, where the hypothesis of pure dynamometamorphism seems not to be sustainable. On this hypothesis it is incomprehensible how indisputable Trias that is indisputably metamorphosed comes to be overlain by unaltered Lias, although the two formations are similarly deformed. *These facts exist : the problem is to interpret them otherwise than by mere words and epithets.*

The phenomena described above we regard as metamorphic discontinuities. To their interpretation we have applied the idea of diffusion in the solid state, because in this way a sharp limit to the transformations concerned becomes more comprehensible. Metamorphic discontinuities are not a figment of the imagination : they are observed facts. Without referring to our own observations, let us quote an example. Intercalated in the gneiss of the autochthonous Massif of Belledonne are beds of but slightly metamorphosed shales that resemble Carboniferous shales. These intercalations have been recorded by Ch. Lory, Pierre Lory, and more recently by M. Gignoux—all celebrated students of the French Alps—who have called them " schistes carburés ". Is this fact, by no means isolated, not just as extraordinary as the halting of the propagation of metamorphism within a siliceous conglomerate such as we have described ? It was in this way that we were led by the facts to the idea of a front of metamorphism.

3. *Is a front of metamorphism conceivable ?*

Towards the end of his article, Prof. Jones invokes the authority of Michel-Lévy, writing : " As Michel Levy (1892) once aptly commented, in reference to another occurrence at Prarion, it would be difficult to admit that in a concordant series the lower beds had been felspathized and transformed into a gneiss, while at a few metres above the strata remained in the condition of unaltered sandstone." In quoting this comment, made more than half a century ago, Prof. Jones, with a

singular lack of caution, enters the domain of " front " petrology. The conception of a " migmatite front " and a " metamorphic front ", first introduced into geology by Wegmann, has now been well known for about fifteen years. It was with this question of fronts that we were concerned in our paper, to which Prof. Jones takes exception. In this paper, we described our careful observations of the contact between the Trias and the crystalline schists of Beaufortin, illustrating our observations with photographs. We should particularly like to draw the reader's attention to the figures on Plate IV of our paper, and to the corresponding discussion. In order to avoid any misunderstanding, we think it desirable to quote an extract from our paper, which we had thought to be scientifically objective, and which does not seem to us to deserve the censure it has drawn from the " authorities ".

" Nos observations ont mis en évidence une certaine continuité de métamorphisme régional, en ce sens que l'on retrouve un très léger métamorphisme commençant dans des parties du conglomérat qui paraissent intactes, mais il n'en reste pas moins que, dans certains cas, aussi bien l'œil nu que le microscope montrent une véritable discontinuité, dans d'autres un passage progressif sur quelques centimètres au plus entre une roche complètement métamorphique et une roche à métamorphisme à peine naissant. Cette discontinuité se produit tantôt à la base du conglomérat lui même, tantôt à l'intérieur de celui-ci. Nous nous voyons obligés de conclure, si extraordinaire que cela puisse paraître, dans le cas particulier, a un arrêt brusque de métamorphisme régional, à l'existence d'un véritable " front " de métamorphisme régional, selon l'expression employée par Wegmann pour la migmatisation, ce qui fait d'ailleurs perdre au mot régional sa portée."

When we wrote these sentences we were fully conscious of the flutter we were going to arouse, but intellectual honesty led us to proceed. We added :—

" Nous croyons que la notion de la possibilité d'arrêt brusque du métamorphisme régional est fondamentale de par ses conséquences en tectonique et estimons indispensable que soient entreprises sur de nombreux points les études de grand détail qui seules peuvent permettre de tirer de premières conclusions légitimes, à l'encontre des examens rapides qui auraient conduit à des conclusions inverses dans le cas qui nous occupe."

We should like to draw our critic's attention to a few references from an actively growing literature with which he appears to be unfamiliar. In the realm of general metamorphism, J. Canet,[1] in a study of Western

[1] This study of J. Canet's is full of lessons. In fact, he begins his remarks about the metamorphism of this region, which is part of the " Arc alpin " in the broad meaning of the term, as follows : " Depuis Philippson, on

Anatolia, demonstrates the importance of a " front de métamorphisme venu de l'Est, d'âge post-crétacé et antélutétien, qui remanie en même temps certaines roches déjà métamorphisées à l'hercynien ". In describing the passage of a normal sedimentary series to a metamorphic facies he adds : " Or ce métamorphisme a bien englobé les lambeaux de calcaire de Demerci et de Kula, mais quand il s'attaque aux puissantes masses du synclinorium Sindirigi, Göges, Kemalpasa, il semble s'y étouffer ; tout se passe comme si les calcaires massifs faisaient un écran ; en distance verticale 100 à 200 m. suffisent."

Passing now to another branch of the subject, which all petrographers regard as being connected with the phenomena of general metamorphism : M. Lamotte and P. Routhier found that on Mount Nimba (Western French Africa) fine-grained magnetite-bearing quartzites formed a barrier to transport of material and to migmatization ; only in one case, with the aid of the microscope, were they able to detect a feldspathization of the base of the quartzite series. They point out that the example " *ne doit pas être perdu de vue dans les futures synthèses du métamorphisme* ".

In the domain of granitization—which is another aspect of metamorphism—Ch. Barrois a long time ago drew the attention of geologists to the occurrence in Brittany of beds of quartzite that remain intact within a milieu of granitized rocks. In the Pyrenees, examples of strips of but little metamorphosed limestone or argillite in the granites of Ariège and of the region of Cauterets are well known from the classical works of A. Lacroix. G. Astre has recently described a new and particularly clear example from the Pyrénées Luchonnaises. Similarly, in Iran, A. Rivière has described the sudden cessation of granitization in the Anti Elbrouz region, against limestones that are hardly metamorphosed.

For a general review of the works of specialists, we would refer the reader to H. H. Read's Presidential Address to the Geological Society of London, in which an excellent list of references is cited. In this address, although showing towards Backlund's conclusions and ours an attitude " of plain peasant caution ", Read declares of the conception of " fronts " in general : " I am certain it has come to stay." It is difficult for us to understand why Read finds the facts we observed less acceptable than others, since in every example the same question is involved : that of fronts of metamorphism. In this connection we should especially like to refer to a report by A. Holmes and D. L. Reynolds of a front of metasomatic metamorphism in Donegal.

considère généralement le Massif de Mendérès comme un môle crystallin très ancien sur lequel sont venus buter des plissements alpins, sans nouveau métamorphisme. En réalité, les séries primaires et crétacées sont reprises par un front de métamorphisme . . . "

Amongst other interesting observations these authors describe the partial transformation of quartzite into mica-schist, within which relics of sharply bounded quartzite still remain (p. 46). With regard to the principles involved, we are unable to see any difference between this example and our own.

It seems then to emerge from this brief review that : (*a*) condemnation of the notion of a front of metamorphism or of metasomatism does not affect the facts that have to be explained ; (*b*) quartzites, probably because of their constitution, are able to act as barriers to metasomatism, and knowledge of this fact does not rest on our observations alone.

4. *What is to be thought about the Hercynian unconformity in the Autochthonous Alps ?*

We have already reported the opinions of eminent Swiss geologists. It appears that the observations on which these opinions were based have since been lost sight of, nobody having explained them in accordance with the purely mechanical theory of Alpine orogenesis that has hitherto prevailed. This theory, moreover, has provided no explanation as to why the Hercynian unconformity of the autochthonous massifs gives place in the Mesozoic synclines, between Aar and Gothard, to a " pseudo-concordance ". What is even more curious is that the schistosity of the crystallines, so " adaptée " to the synclines in Alpine times, is yet continuous with that of the whole massif, including those regions where the unconformity is observed.

On the northern border of the autochthonous massif of St. Gothard, where the crystallines, Permo-Carboniferous and Trias lie conformably, E. Niggli finds it impossible to trace a limit, within a distance of 20 to 100 metres, between the Permo-Carboniferous and the crystallines, saying that " a strong diaphtoresis has brought about a convergence of aspect ".

Throughout the Alps, according to many Swiss authors, dynamometamorphism is responsible for all, is all powerful, develops all the minerals—feldspars, amphiboles, etc.,—generates mica-schists, gneisses, and even veins in the Pennine nappes, and is also regarded as responsible for the formation of the " schistes lustrés ", although it should be noted that by the French School these are assigned to regional metamorphism. No one is any longer able to distinguish the difference between the action of dynamometamorphism and that of regional metamorphism. When certain facts are too difficult to interpret by dynamometamorphism alone, then a " telepneumatolysis " is invented and brought to the rescue. Thus, hypotheses accumulate, becoming more and more numerous in order to bolster up the theory against the brutal facts. And whoever dares to doubt the dogmas of the authorities

is declared anathema. All are required to agree that in no case is the dogma of the Hercynian unconformity contradicted by the facts ; if it be recorded that the Trias is metamorphosed and sometimes contains minerals like those of the subjacent crystallines, then it must be that the authors have wrongly observed ; and if the unconformity passes to conformity within the synclines, then this is a perfectly normal phenomenon, with an obvious interpretation.

We cannot resign to this iron discipline by which progress is stemmed. There is still much to be learned about Alpine geology, and detailed petrographic observations accompanied by the study of thin sections are indispensable for the enlargement of our knowledge, because only in this way are we able to take a further step towards the evolution of more satisfactory theories.

One fact is certain : at many places in the Alps the Hercynian unconformity assumes a very different and more complex form from that which is classical in the Massif Central and " Bassin parisien " : the observations we have cited are sufficient to demonstrate this. It is also a fact that the Mesozoic, particularly the Trias, has been metamorphosed in many parts of the Alps, including the autochthonous massifs. Consequently, the Alpine orogenesis had metamorphism, whether dynamic or not, as a concomitant. Further, this dynamometamorphism was accompanied by chemical diffusions, for without this there cannot be even sericitization of plagioclase or chloritization of biotite. By detailed observations we have shown, for rocks near the glacier at Tré-la Tête that could be considered as typically dynamometamorphic, that chemical diffusions have taken place between the different layers. We deduced that there is no sharp division, but rather continuity, between dynamometamorphism and regional metamorphism. We have already mentioned our view concerning this important problem and its application to Alpine geology.

In the presence of this certain Alpine metamorphism, it is in no way extraordinary if the crystallines of the autochthonous massifs owe their present form, at least partially, to more or less deep-seated transformations of Tertiary age. This conclusion is in no way affected by the question as to whether these rocks were previously crystalline or not.

We do not deny the unconformable transgression of Mesozoic seas over a partly crystalline Hercynian floor in the external Alps. To conclude from this, however, that the crystallines or " cristallophylliens " remained essentially unchanged from the end of the Primary era right through the Alpine orogenesis, is a gigantic step that we cannot take, as others do, by ignoring the actual evidence. Indeed, in the present state of knowledge, we see no other way of interpreting some of the facts than as a front of metamorphism.

" La conjoncture est grave, puisqu'elle remet en question une

fois de plus des dogmes très admis. Nous ne nous sentons pas le droit de conclure et ceci nous paraît appeler de nouvelles observations." Thus we concluded the note that drew upon us the thunders of Prof. Jones, and we still see no reason to modify this, on the whole, extremely prudent conclusion.

We shall, without prejudice, give every consideration to possible interpretations of the facts, whether they be observed by others or by ourselves, according to the old or classical theories. We are ready to change our minds if such interpretations are more convincing than our own, and are not merely words or epithets such as " regeneration ", " diagenesis ", or " appels de faciès ". Of immediate interest is the necessity to explain the fact that the Triassic rocks under discussion are metamorphosed, whilst the superimposed Lias is not. In any case, we cannot accept the accusation of having confused obvious Trias with a " peculiar rock formed by decomposition of the old rocks of the peneplain ".

We regret the misunderstanding that has arisen through Prof. Jones's judging at a distance. We are convinced that he would have reached other conclusions if he had referred to the papers written by earlier observers, and if he had fully appreciated the nature and significance of our own investigations.

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A Suggested Origin of Continental Slopes and of Submarine Canyons [1]

By K. O. Emery (University of Southern California)

Abstract

Thrusting along a shear plane at the continental margins may result in a temporary up-bulging of the margins above sea-level. During the time of exposure erosion by streams should have incised canyons which now, after isostatic readjustment of the margins, constitute the widely distributed submarine canyons. Known downwarped peneplains below the surface of continental shelves may have been developed on the bulged margins by long continued erosion. The margins may, thus, have served as sources of some sediments now found on land and believed to have been derived from a seaward direction.

CONTINENTAL slopes and the submarine canyons which incise them have intrigued many geologists, who have offered a number of ingenious explanations for both. Evidence for and against each of the many suggestions for submarine canyons have been discussed by Shepard (1941) and others. The hypothesis most successful in explaining the physiographical and lithological relationships appears to be that of subaerial erosion during a time of exposure of the present sea floor. Those who object to this hypothesis base their objections almost exclusively on the belief that the growth of Pleistocene glaciers could not have resulted in a sufficiently great lowering of sea-level to have exposed the required amount of sea floor.

The continental slope in which the canyons are cut is relatively straight and generally parallel to mountain ranges on land and to trenches or fore-deeps on the sea floor, if the latter are present. Tectonic hypotheses of the continental slope, however, have had to explain the low declivity—generally only 4° to 5°. The four main suggestions for the origin of the slopes are : fore-set beds of wave-built platform, normal step-faults with seaward dip mantled by sediment, normal fault with a very low seaward dip, and downwarped peneplain (Shepard, 1948*a*). The first of these is pretty well eliminated by the common presence of rock and gravel at the top of the slope ; the others will be discussed below.

Earthquake foci in seismically active areas along the entire west side of the Pacific Ocean and on the east side of the Pacific from the Caribbean Sea along the Andes to near Antarctica are shallowest near the continental slope but they increase in depth in a landward direction (Gutenberg and Richter, 1937, 1941 ; Hess, 1948). The foci may indicate the position of a 45° shear plane extending possibly as deep as 700 kms. The dip of this plane is directly opposite to that proposed

[1] Contribution of Allan Hancock Foundation.

for faults of the continental slope. Perhaps, however, both fault planes may be related in the manner proposed by Willis (1936) and others for the structure of rift valleys. Through the action of compressive forces the plateau blocks on either side of the graben are believed to slide up on the underlying wedge floor. The unsupported forward edges then slump down as normal faults dipping toward the graben. Could it be possible that the continental slope is a feature of somewhat similar origin, thus resolving the two opposing sets of presumed faults ? Both the rift valleys (Bullard, 1936) and the trenches beyond the active continental margins (Hess, 1938 ; and others) are characterized by negative gravity anomalies, thus suggesting the presence of a light mass being over-ridden. One may suppose that thrusting along a deep shear plane at the continental margin might result in an upward bulging of the continental shelf and slope perhaps to above sea-level. At this time the cutting of stream valleys and possibly even peneplanation could take place. Sediments derived from the bulged margin should be transported seaward from one of its sides and landward from the other side. Several instances of sediments believed to have come from a seaward direction from former highlands now absent have been recognized in the stratigraphic column on both the Atlantic and Pacific coasts of the United States and elsewhere. Among the former ancient highlands are the well-known Appalachia, Cascadia, and Catalinia. Eventually the margin may be lowered by step faulting, or more probably by gradual downward warping of the surface.

The marine work of Ewing and his associates (Miller, 1937) indicates the presence along the Atlantic coast of the United States of a peneplain downwarped to a depth of about 10,000 feet below the continental shelf ; this may be the pre-Schooley peneplain inferred by Ashley (1935). This old peneplain on basement rocks bends upward again near the present continental slope, perhaps indicating a renewal of up-bulging. Similar downwarping off the coasts of the Gulf of Mexico (Storm, 1945), off south-west Africa (Veatch and Smith, 1939), and off England (Bullard and Gaskell, 1938) appears to exist. Perhaps one should thus expect that canyons have been cut into the bulged continental margins by subaerial processes and that they are now downwarped to a position far below the probable depth of glacial lowering of sea-level. The canyons may have been cut during more than one cycle of upward bulging and emergence followed by downwarping and submergence.

Shepard's (1948*b*) recent chart of the distribution of the canyons shows that nearly all are on the continental margins or on tectogenes, which are thought to mark the edges of continental masses. One might well object that many of the canyons are on continental margins that are not seismically active at present, for example all around the Atlantic

Ocean. Could these canyons be older than canyons around the Pacific, and formed when the Atlantic continental slopes were more active than now ? Other exceptional canyons are those of the Hawaiian Islands, but perhaps these were cut into an isostatically sinking mass. As additional objection is that the latest movement along all continental margins is downward or submergent, but it may be argued that this is the result of superposition of returning glacial melt-water upon the movement.

Other workers (Du Toit, 1940 ; Umbgrove, 1947) have recognized that bulging of the continental margins may provide a reasonable means of exposing the sea floor to subaerial erosion, but the proposed methods of exposure have not met with general acceptance. This explanation is offered as a generalization to be tested, in the hope that it will not merely add to the present confusion of hypotheses.

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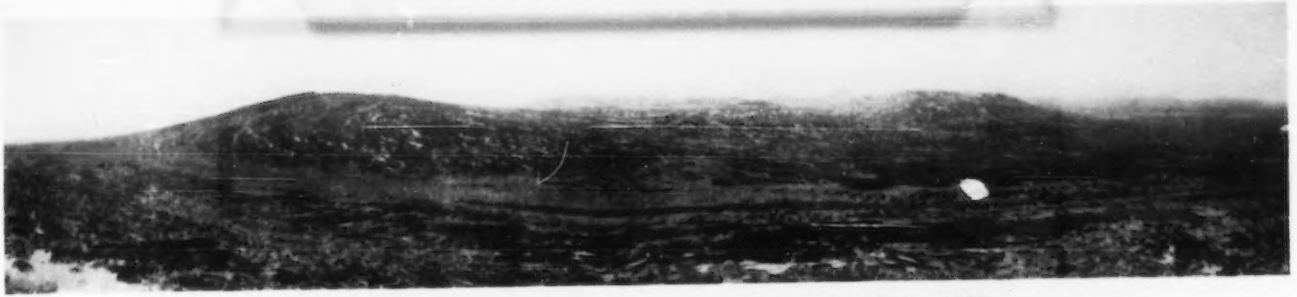


FIG. 1.—DARWIN'S "STONE RIVER" AS FIGURED BY ANDERSSON (1906).

FIG. 2.—A RECENT MUD FLOW, DECEPTION ISLAND, SOUTH SHETLANDS.

Stone Runs of the Falkland Islands

By J. R. F. Joyce

(PLATE VII)

Abstract

The suggestions made by previous writers are summarized, and Andersson's Solifluction Theory is discussed in detail. Reasons are given why the author believes that solifluction alone could not provide the explanation. Stone runs are shown to be largely an accident of past climate and the structure and lithology of the Upper Quartzite Series rocks. A note on the economic aspect of stone runs is added.

SINCE the earliest published descriptions, the stone runs of the Falkland Islands have been a source of interest and speculation to geologists and physical geographers. When Gunnar Andersson (1906) proposed the solifluction theory of their origin, the matter seemed closed, for this theory has been generally accepted as satisfactorily accounting for their formation. Unfortunately the Falkland Islands are remote and little visited by scientists who have the time and opportunity to study the phenomena. This paper is based on studies of the stone runs of East Falkland. There was no opportunity to visit West Falkland, but Falkland Islanders who could speak from personal knowledge were questioned as to the localities where the runs occurred. Their reports were collated and recorded on the Admiralty Chart, which is the only map available. The time now seems opportune to bring to notice some serious objections to Andersson's theory and provide a further explanation.

No observer attempted either a scientific description or an explanation until Darwin (1846) published his excellent account of the stone " river " at the head of Berkeley Sound (Plate VII, fig. 1). This was to him so awe-inspiring that he invoked the agency of a catastrophic earthquake as the explanation. It was left to Wyville Thomson (1877) to suggest something nearer the truth when he said that many factors took part in their formation. He selected alternate drying and saturation and vegetation decay as causing hill creep, which he took to be the transporting agent. From Darwin's and Thomson's descriptions James Geikie (1894) suggested that the stone runs were comparable to the rubble drift of southern England, and that they are essentially a product of tundraic conditions where " freeze and thaw " attains a maximum effect. Andersson (1907) carefully studied the suggestions made by these authorities and incorporated such of their ideas as seemed to him sound in his paper. He made a most painstaking scientific study and map of the stone " river " described by Darwin.

Darwin (1846) selected for his description the stone run most like a river ; had Andersson selected a less river-like example in another area he might have had cause to modify his ideas considerably.

Andersson proposed that the stone " rivers " are caused by the flow of supersaturated soils which bore with them the stone blocks. This process he says is still operative in the Falklands, and he shows a photograph of a present-day mud flow at Port Stevens, on West Falkland. Baker (1922), unable to find himself in agreement with Andersson, postulated local ice caps with quartzite ridges as nunataks. From these nunataks, he says, frost action detached large blocks which " slithered down the icy slopes ".

Taking the stone runs as a whole there is some truth in each of the above-mentioned theories, but no single one will account for the whole process. Since Andersson took careful account of all the previous work this discussion can profitably begin with his theory.

Andersson, in describing the modern flows, admits that they are smaller than those necessary to produce stone runs as large as the Darwin " river ". Further, he suggests that the flows only occur where the slopes are steeper than they are at the head of Berkeley Sound, and where vegetation is scarce and trickling water has saturated a section of a steep slope. He compares the Falkland Islands with other areas where this is operating, and rightly concludes that solifluction can mostly be expected where tundraic conditions have existed for a considerable time.[1] He also recognizes that the stone runs of the Falklands extend over a greater area than is at present exposed, since they are masked by vegetation.

His theory of the formation of the stone runs can be criticized in the following argument. Not all stone runs lie progressively downhill ; there are some notable examples which lie across the tops of low dome-like hills. This feature is mostly noticeable where the dips of the underlying rocks are low. On the boundary of the Devono-Carboniferous Series and the Lafonian Series no stone run flows over the Lafonian. Since Wickham Heights, which overlook the Lafonian, are the highest range on the Islands, it is here that one would expect the greatest development of the stone runs. This is not the case. The material of the stone runs is always quartzite, obviously parted on the jointing and bedding planes, and blocks can attain in many instances considerable size. Some measured on the Moody Valley stone run are estimated to contain more than 40 tons in a single block. Such blocks would surely have sunk, under gravity, to the bottom of any viscous mass more rapidly than they could have been transported laterally. Further, they are so numerous that they would have soon jammed immovably on the gentle slopes, where friction and the

[1] At some risk of being taken to task by climatologists, I propose using the adjective tundraic ; using it essentially to denote the effect of tundra-climate temperature-changes (" freeze-thaw " phenomena) irrespective of questions concerning precipitation amounts, although I am aware that the tundra is a continental feature.

gravitational forces exerted on each block would have been greater than the force exerted by the moving mass of soil. No section of a stone run from the surface to the sole is available for examination, but all writers are agreed by implication that they are of considerable thickness. How the required thickness of soil necessary to transport such a volume of stone could have accumulated during the time tundraic conditions were existing is difficult to imagine.

If mud had accounted for the movement of the blocks, it is certain that not everywhere would the flows have occurred at the same time. Thus, overlapping of the flows would have taken place. Present-day mud flows have a typical form, as is shown by a very recent one seen in Deception Island (Plate VII, fig. 2). The flow is small, but nevertheless it must illustrate the action of larger flows. The photograph shows : the steepness of the slope from which the flow came, the checking of the flow as soon as the grade is reduced, and the peculiar " mine-tip " appearance of the moved material. The transportation of moderately large-sized blocks for short distances is also illustrated. It is believed that the reduction of the grade was not the only factor which brought the flow to a standstill. On the distal and lateral sides of the flow there were erosion gullies, which are evidence that the flow, by its movement, drained itself. By this reduction of water content the flow became too viscous for mobility. Mud flows must drain too rapidly to flow for several miles over slight slopes, even if we postulate an annual movement under " freeze and thaw " conditions. On some slopes the sheets of stones are continuous. On others it is not difficult to imagine, nor beyond the bounds of possibility, that the boulders exist over large areas which are now masked by vegetation. Indeed, the hummocky surface and isolated exposed boulders provide definite evidence for this belief.

For complete understanding of the stone runs of the Falklands some account of the general geology of the area is necessary, since their origin appears to be connected with the tectonics and the lithology, and perhaps most particularly with the Antarctic glaciation.

The succession of the rocks has been worked out by Andersson (1906), Halle (1912), and Baker (1922), as being :—

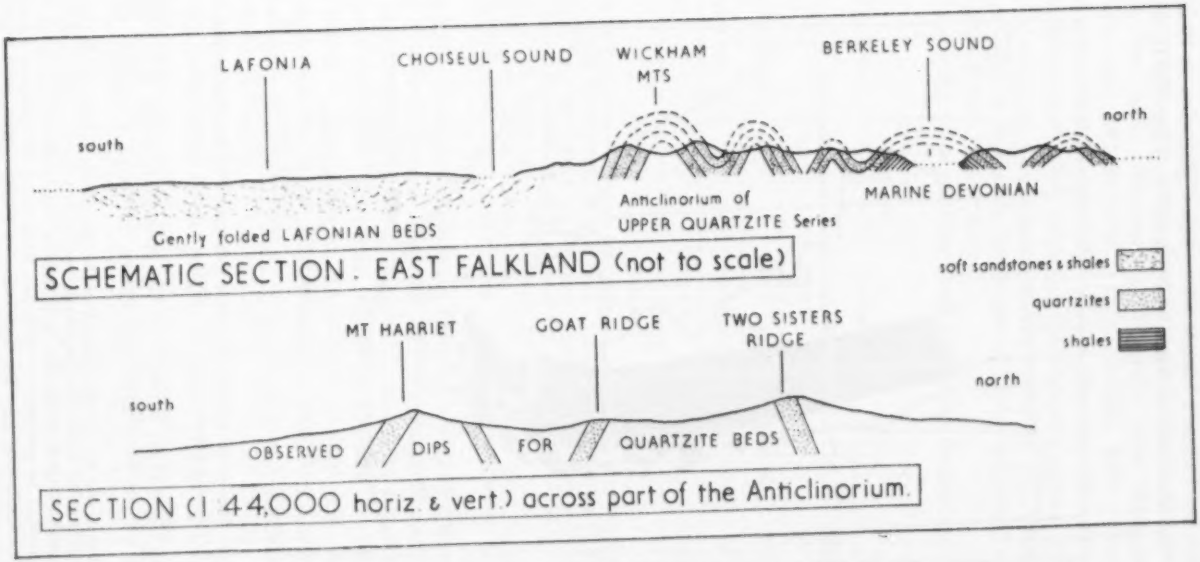
Lafonian (Gondwana)	Upper Series	Upper part Lower part
	Lower Series	Lafonian sandstones Lafonian Tillite
	Unconformity	
Devono-Carboniferous	Upper Quartzite Series Marine Devonian Series Lower Devonian Series	
	Unconformity	
Archean . . .	Cape Meredith Series	

The distribution of these rocks is shown on maps in the works quoted. Observations on East Falkland show that stone runs are confined within the boundaries of the Upper Quartzite Series. The plotted occurrences from West Falkland confirm this. Stone runs, for reasons which will be shown, are not everywhere found within these boundaries.

The tectonic pattern of the Falklands is not known in great detail, and adequate topographical maps are lacking. The general strike is east and west in the northern part of the islands, but Falkland Sound is a line of discordant strike. Whether or not there is much faulting and overthrusting cannot be discussed here, but the distinct rucking between the tectonic lines at the north end of Falkland Sound can scarcely have occurred without some enormous accommodation in the rocks. Over the main outcrop of the Quartzite Series on East Falkland the structure is that of a sharply folded anticlinorium, with a distinct suggestion of some strike faulting. The individual folds in the anticlinorium are of small amplitude, especially where the quartzite bands are thin. In the field, rocks can be seen dipping to both flanks of the anticlinorial axis. In the centre the dips are high, becoming much lower towards the north than they are on the south side of the axis. The sections in Text-fig. 1 demonstrate this.

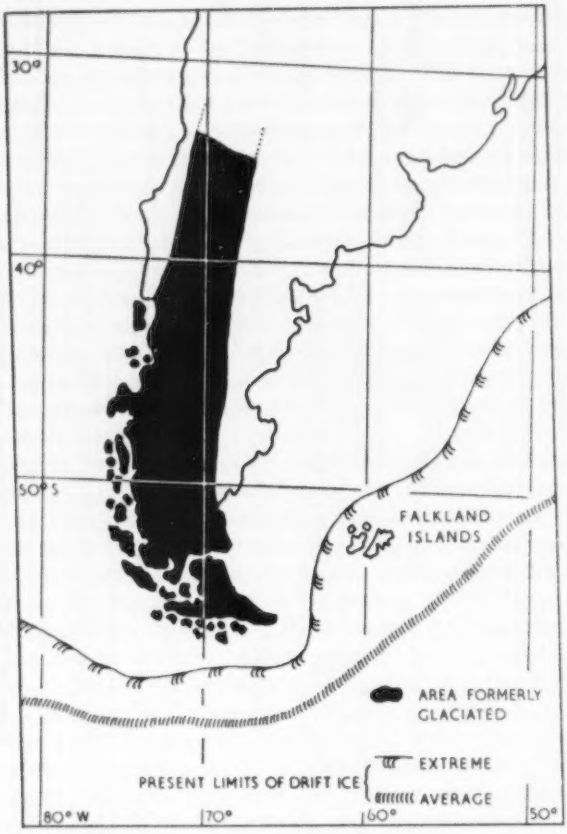
Within the Quartzite Series the two main rock types are quartzites and shales. Some of the quartzites appear in hand specimen to be fairly well recrystallized, while others have the appearance of being siliceous sandstones. Between these two types there are all gradations. Where the quartzites are sharply folded, saddle-shaped vughs have opened on the bedding planes at the crowns of the folds. Quartz crystals have been deposited in these vughs. In vughs exposed in dissected anticlines quartz crystals can be found lying loose and separated. Here is an easy road for attack by the pressure splitting action of ice formed in already made partings in the solid quartzites. The shales, both within the Quartzite Series and within the Marine Devonian, show a cleavage lying at variable angles to the laminations. The angles appear to vary with the intensity of the folding. The cleavage causes the shales to break very rapidly into trapezohedral fragments, often less than an inch and rarely more than a few inches in length. The fragments are often highly iron-stained and, as the shale is fairly rich in pyrites, it weathers easily.

In the Falklands, as remarked by Andersson (1906), evidence of glaciation is entirely lacking. There are no ice-moulded forms, striations, roches moutonées, moraines, or tills. A few erratics, however, are found, but these are confined to the shores, and may be explained as having been transported by ice rafts. There is no evidence that the Antarctic ice ever advanced to South America, and the glaciation there was only an advance of the Andean ice cap ; and, indeed, it seems



TEXT-FIG. 1.—Geological sections across East Falkland.

clear that the Falkland Islands also escaped glaciation in Pleistocene times. The maximum advance, as suggested by Flint (1947), is shown in Text-fig. 2.[1]



TEXT-FIG. 2—Maximum extent of former Glaciation of South America (after Flint) and present limits of Drift Ice (after Times Atlas).

Further, there is no evidence in the geology to relate this advance with that of the Northern Hemisphere, although Caldenius (1932)

[1] The form of the west coast of South America, south of Lat. 42° S. (i.e. the most northerly point where the glaciation reached sea-level) contrasts strongly with that north of 42° S. I can find no reference to it in the literature but I am strongly tempted to compare it with the west coast of Graham Land, as described by Holtedahl, and the west coast of South Island, New Zealand, south of Lat. 44° S.

attempts to place the South American glaciation on the De Geer Swedish time scale. Simpson (1930) would make them contemporaneous, and Milankovitch (1930) considers them slightly dissynchronous, while Brooks (1949) thinks they may be without any time-relation to each other. Although the tempering effect of the South Atlantic and South Pacific water masses prevented the Antarctic ice from reaching the Falklands, the climate of the Islands must have been more rigorous than that of to-day.

The following table provides some interesting comparisons.

Temp. Means	Latitude 50° S. (sea average)	Falklands	South Georgia	Bear Island
Yearly	41·9°	42·8°	37·8°	29°
Maximum monthly	46·9°	49·3° (Jan.)	43·5° (Feb.)	41° (Aug.)
Minimum monthly	37·4° (July)	36·7° (July)	27·9° (July)	17° (Mar.)
Range. . .	9·5°	12·6°	15·6°	24°

Notes.

(1) All temperatures in degrees fahrenheit.
(2) Falklands Station is Cape Pembroke ; (data from Brooks, 1920).
(3) South Georgia Station is Grytviken ; (data from Brooks, 1920).
(4) Bear Island quoted from M.O. 4466 (7), vol. 11, pt. 7, 1941.
(5) Lat. 50° S. quoted from Meinardus, 1938.
(6) South Georgia has three months with the average temp. below 32° F.
(7) Bear Island has seven months with the average temp. below 32° F.

The table shows just how little the Falkland Islands climate of to-day would have to deteriorate to produce conditions of " freeze and thaw ". The amount is not outside that probably caused by the advance of Antarctic glaciation, and Bear Island is included to provide a comparison. This advance must have pushed the Antarctic Convergence further to the north than it is to-day. At the present time the Falklands lie between the extreme and average limits of drift ice, as is shown in Text-fig. 2. The probability is that they then lay between the average limit of drift ice and the edge of the ice pack. How near they lay to the edge of the pack is a matter of speculation.[1] It is probable that the large water masses, circumpolar currents, and prevailing winds would effectively disperse the pack ice long before the latitudes of the Falklands were reached. What, however, seems a reasonable assumption is that the waters of the Falklands in those times were often quite polar because of the increased quantity of drift

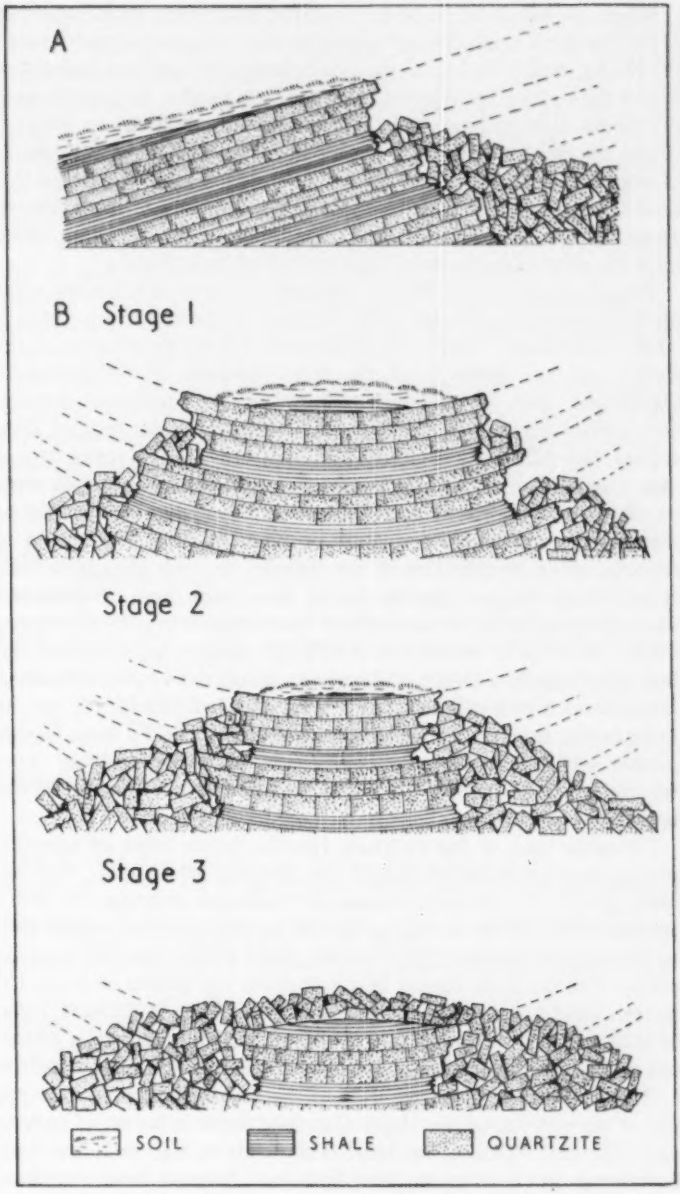
[1] In the hope that information on this score might be deduced from ice rafted erratics, I searched the literature for records. None was found, and surely here is a useful line of research.

ice. At the same time, the polar front must have been farther north, bringing the Falkland Islands more often into the region of cold air behind the front than it does to-day. When more is known about the peats, the soils, the raised beach deposits, and the West Point Island Forest Bed this past climate may be less of a matter of speculation.

The preceding argument provides evidence for the main causes of the formation of the stone runs. To picture the method of formation is not difficult when we remember firstly, the rock series composed of quartzites, resistant, jointed, and bedded, contrasting with the shales, easily weathered and already in small trapezohedral fragments; secondly, the small sharp composite folds of the anticlinorium; and thirdly, a possibly long period of tundraic or near-tundraic climate.

If the climate was tundraic, " freeze and thaw " would be operating at its maximum, and as a consequence the quartzites and shales would show differential disintegration. The quartzites, not being readily subject to weathering and being almost homogeneous, would only be affected along the major jointing and bedding planes, which were already opened by the folding for the attack by frost. Because of the frequency of the individual folds, at least near the axis of the anticlinorium, the attack would take place at many points at the same time. The shale already in small fragments would, in addition to frost action, show some weathering, and the fine material produced by this would be removed rapidly by " freeze and thaw " action. The proportions of quartzite to shale are not known, because of the poor exposures due to masking by peat, but it is believed that the shales are subordinate. Where shales underlie the quartzite, there would be a progressive undercutting and the frost-parted blocks would in the course of time be detached from the scarps. The blocks would remain almost in the position where they fell, and thus would carpet the area over which the scarps retreated. The size of the blocks would be almost entirely controlled by the disposition of the jointing and bedding planes, since frost shattering would have little effect on such a homogeneous material as quartzite, except where it could penetrate into fractures and partings.

On the dip slopes there would be no detachment of the quartzite, as is shown in Text-fig. 3A and, where the dips are small and folding gentle, there would similarly be no breaking up of the strata. This would account for the areas within the Upper Quartzite boundaries, which are free from stone runs, and also for the absence of stone runs on the southern flank of Wickham Heights which is a dip slope. Several dispositions of the strata could account for the occurrence of stone runs which lie across the tops of gentle slopes. In Text-fig. 3B the stages for a gentle syncline are shown. The relation of stone runs to minor tectonic features is also illustrated.



TEXT-FIG. 3.—Diagrams illustrating formation of Stone Runs.

Where the blocks are in widely assorted sizes or are more uniformly of smaller size it is not difficult to imagine fine material trapped between the blocks, finally leading to the establishment of soils and vegetation. Where the jointing and bedding provided large blocks, the gaps between the blocks were so large that the fine material could not be trapped. Where the stone runs were of greater thickness, vegetation was unable to establish itself deep in the stone runs owing to lack of light, so that even this agent was unable to assist in the trapping of fine material. There has therefore never been any vegetation over these areas, which form the almost unique stone runs of the Falkland Islands.

There is some truth in Wyville Thomson's theory of hill creep, since the blocks must have moved short distances in settling down. Geikie's " freeze and thaw " theory would account for the detachment of the blocks and the removal of the finer products of disintegration. Solifluction, while admittedly a powerful agent of subaerial denudation, cannot explain those stone runs which cross the tops of hills. A mud flow sufficiently large and powerful to move millions of tons of rock over 4 kilometres down grades of a few per cent requires some corroborative evidence by solifluction in action to-day. Had it played even a major part in the formation of the stone runs, overlapping of deposits, crude stratification of the deposits, or even ploughing back of previously formed deposits would have been seen. Solifluction played its part, but in a minor rôle to that assigned to it by Andersson (1906). If the colder period was of sufficient intensity to form cyclic ice, then Baker's picture (1922) of the quartzite nunataks being exposed to intense frost action will be true, but the detached blocks will not, as he supposed, have " slithered down the icy slopes ". To those factors another of powerful local action should be added, namely frost heaving, which may possibly account for some of the up-ended blocks figured by Andersson.

The stone runs of the Falkland Islands, beside being of scientific interest have an economic aspect not generally recognized. To-day, when horses are the main means of transport between the sheep stations, they can be a serious bar to communications where they necessitate long detours. They will also make the construction of roads costly. For the same reason they will make the finding of a site for any proposed aerodrome near Port Stanley difficult. At present, those parts of the stone runs which are devoid of vegetation sterilize a greater part of the upper quartzite outcrop than is realized. On sheet 1B of the 1 : 25,000 G.S.G.S. Map No. 4456 of the Falkland Islands, 17·8 per cent of the outcrop of the Upper Quartzite Series is barren of vegetation. For East Falkland the figure is probably as high as 15 per cent. No estimate can be given for West Falkland. None of these considerations is, however, serious when compared with the following effect.

The soils of the Falklands have such low *p*H values that only potatoes and oats can be grown as field crops, and for this to be remedied drainage operations would have to be undertaken. Since much of the area where stone runs occur is carpeted by quartzite blocks even under the soil cover, drainage schemes will be difficult to devise. Thus, soil development on these areas will be greatly hindered. It is estimated that this condition will apply to 25 per cent of the total area of East and West Falkland.

The stone runs of the Falklands are, therefore, largely an accident, both of past climate and the structure and lithology of the Upper Quartzite Series. Hill creep, freeze and thaw, frost heaving, and solifluction all contributed to the formation, but their importance must be kept in true perspective.

The writer wishes to express his indebtedness to Mr. Cameron Ovey, of the British Museum (Natural History), for a critical reading of the manuscript and many valuable suggestions, and to Professor J. Gunnar Andersson for permission to reproduce Fig. 1, Plate VII.

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The Fauna of the Rhaetic Beds in South Nottinghamshire

By M. R. W. Johnson (University of Nottingham)

Abstract

The Rhaetic section at Newark is described and compared with sections further south. The Lower Rhaetic is locally sub-zoned by constant bone-bed horizons. In the south-west of the area a much richer fauna occurs and the zone-fossil *Pteria contorta* appears lower in the succession. The explanation offered is that a lagoon phase existed in the more northerly district.

ALTHOUGH the Rhaetic beds of Nottinghamshire have been previously studied by E. Wilson (1882) and the Geological Survey (1909), when exposures existed along railway cuttings and in quarries, no accounts of the precise positions of important fossils were given, and most of these exposures are now grassed over. Therefore old sections at Normanton Hills, Stanton on the Wolds, Barnstone, and Newark have been re-examined. In addition relatively fresh exposures at Bunny Hill and Blue Hill (near Cropwell Bishop) were examined for confirmatory evidence.

The section at Beacon Hill, Newark (few yards south of Sleaford road), was excavated, and the faunal sequence was as follows :—

	ft.	*in.*	
Black Shale		9	
Black Shale	1	4	*Pteria contorta* rare.
Shale and sandstone	2	0	
Sandstone		1	*Protocardia rhaetica* rare.
Shale		2½	
Sandstone		1½	*Protocardia rhaetica.*
Shale with sandy patches, and a band of intermittent nodules	2	2	*Protocardia rhaetica*, worm-bores.
Sandstone		1	*Protocardia rhaetica* rare.
Shale	2	1	*Protocardia rhaetica* rare, lignite.
Shale		4	*Gyrolepis alberti, Acrodus minimus, Saurichthys ?*
Shale	1	4	
Shale	1	4	
Shale		6	*Gyrolepis alberti*, fin rays, and scales.
Shale and sandstone	1	11	Coprolites.
Shale	1	3	Fish teeth and scales.
Tea-green marl.			

A comparison of the above section with that at Normanton Hills (tunnel cutting, 1½ miles S.S.W. of East Leake, and 24 miles S.W. of Newark) is instructive. The section there was described by Mr. M. Browne (1895), but the following gives the exact faunal succession in the Lower Rhaetic :—

		ft.	in.	
BED WITH CONTORTA	Black Shales	1	6	*Pteria contorta, Protocardia rhaetica, Schizodus ewaldi, Anatina praecursor, Actaeonina valleti?* Stoppani.
	Black Shales	2	0	*Pt. contorta, P. rhaetica, S. ewaldi, Myophoria inflata, A. praecursor, Pecten valoniensis, Pterophyllum brevipenna?*
	Shale	4	6	*Pt. contorta, P. rhaetica, Pecten valoniensis,* rare.
	Shale		6	*Gyrolepis alberti, Acrodus minimus, Nemacanthus monofilifer, Saurichthys acuminatus,* etc., with *Pt. contorta.*
	Shale	1	4	*Pt. contorta, P. rhaetica.*
	Shale		9	*G. alberti, Rhysosteus oweni, Pt. contorta, P. rhaetica.*
	Shale	1	6	
	Shale	1	2	Fish scales and teeth.
	Tea-green marl.			

The beds of this section can be grouped in the following manner :—

	ft.	in.
3. Upper Pteria Beds	8	6
(Bone-Bed)		
2. Lower Pteria Beds	2	0
(Bone-Bed)		
1. Pre-Pteria Beds	2	10

The two beds of vertebrate remains consequently separate the black shales into three distinct faunal divisions. In addition these beds occur at the same horizons in all exposures of the area, including Newark (see section), and so may mark sub-zonal boundaries. Owing to north and south thinning, away from a maximum thickness at Bunny, these divisions vary in thickness. For example, the Pre-Pteria Beds are nearly 3 ft. thick at Stanton and only 1½ ft. thick at Barnstone.

On comparing the sections described above it is seen that at Normanton Hills the zone-fossil *Pteria contorta* first entered the area during a pause of deposition (marked by lowest bone-bed referred to above) and increases in numbers to the top of the beds, whereas at Beacon Hill *Pteria occurs* only rarely near the top of the beds, classed as Upper Pteria.

At intermediate exposures the position is similar to that at Newark. For instance, at Stanton the Upper and Lower Pteria beds are generally unfossiliferous, and *Pteria* is absent. While at Barnstone *Pteria* only occurs high in the succession. The shales at Blue Hill were examined but *Pteria* was not found. It might have been expected that *Pteria* would appear simultaneously at all localities, but this is not so.

The fauna at these exposures suggests a possible explanation of the absence of *Pteria*, for it is very impoverished at Stanton, Blue Hill, and Newark, and at Cotham only rare *Schizodus ewaldi* was recorded by Wilson (1882). But the sections at Normanton Hills and Bunny Hill have revealed a much richer faunal assemblage than before.

The following table shows the distribution of fossils in the beds of South Notts :—

	Normanton Hills.	Bunny Hill.	Stanton.	Blue Hill.	Barnstone.	Gotham.	Beacon Hill.
Pteria contorta . . .	bc 3	bc 3			c 2		c 1
Protocardia rhaetica . .	bc 3	bc 3	c 1	c 1	bc 2		c 2
Schizodus ewaldi . .	bc 2	bc 2	c 2	c 1		c 1	
Anatina praecursor . .	c 2						
Myophora inflata . .	c 3						
Pecten valoniensis . .	c 2						
Actaeonina valleti . .	c 2						
Pterophyllum . . .	c 1						

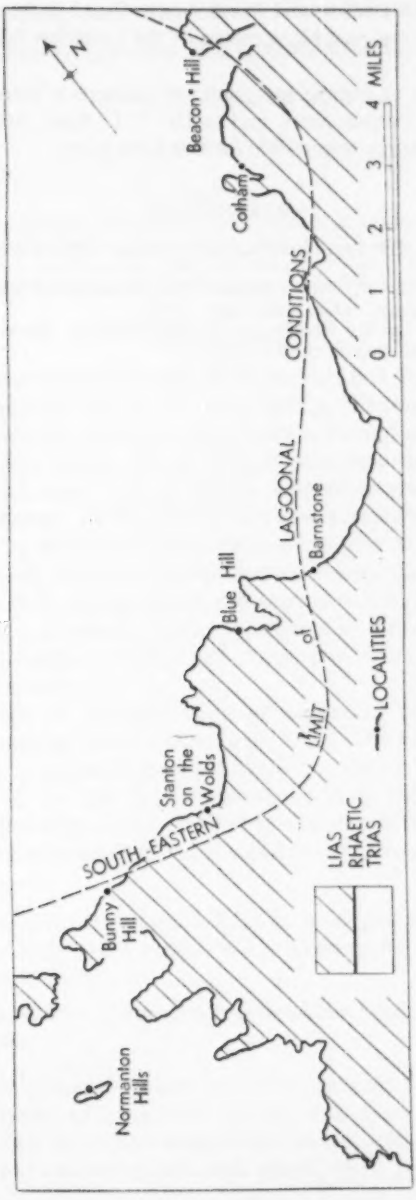
a = Pre-Pteria Beds, b = Lower Pteria Beds, c = Upper Pteria Beds
1 = rare, 2 = fairly common, 3 = common.

The top shale bed at Barnstone and Blue Hill contains scarce fossils, but at Bunny it is richly fossiliferous. Furthermore, at Normanton Hills this bed has yielded eight or nine lamellibranch species, also the plant *Pterophyllum* and the gastropod *Actaeonina*.

Thus, the evidence indicates that the fauna becomes richer south-westwards from a line between Stanton and Bunny. The inference is that the ecological circumstances of the area south of Stanton varied from those to the east and north. It is suggested that this was due to the existence of lagoon phase in the latter district, so that the fauna was more impoverished than elsewhere. At Cotham, extreme impoverishment existed. But at Beacon Hill the occurrence of *Protocardia* low in the succession indicates that the impoverishment was alleviated slightly in the extreme north of the area.

Confirmatory evidence of such conditions at Stanton is seen in a bed of sun-cracked sandstone, which implies very shallow water. Further, the occurrence of lignite at Newark, together with worm-bores and ripple-marks indicating the proximity of land, supports the view. Fuller's earth at Bunny is evidence of deposition in shallow, sheltered waters.

With regard to the upper Rhaetic beds, it might be recorded here that possible current-bedding was seen in a trench-section at Bunny Hill, at the base of the grey marls, while one limestone bed showed fine current bedding and silt-streaking. Rare specimens of *Volsella* cf.



TEXT-FIG. 1.—The Rhaetic of South Nottinghamshire.

langportensis Richardson and Tutcher were found in these beds. This would indicate the possible presence of the Langport Beds in part of the area.

I should like to express my thanks to numerous friends who have assisted me at various times, and to Dr. P. E. Kent, whose help and guidance are mainly responsible for this little paper.

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Olivine-bearing and other Basic Hornfelses around the Insch Igneous Mass, Aberdeenshire

By M. S. SADASHIVAIAH

(PLATE VIII)

ABSTRACT

Basic hornfelses occurring at the contact of slates and olivine-norite are ascribed to influx of Ca, Mg, Fe, Ti, Mn, and P from the gabbro magma into the sedimentary rocks. This kind of basic front, unlike those postulated for acid bodies, has a basic back.

INTRODUCTION

IN the Survey Memoir on the Banff, Huntly, and Turriff districts of North-east Scotland, H. H. Read (1933, p. 160) made brief mention of certain basic olivine-bearing hornfelses occurring at the contact of the Insch olivine-norite with the Macduff Slate series of the Dalradian. These unusual rocks were tentatively assigned to the Macduff Slate side of the contact. In the course of a recent investigation of the whole Insch Mass by the present writer the two localities at which the basic hornfelses occur were re-examined and the results are here presented. Apart from their petrographical and petrogenetic interest these basic hornfelses have a certain topical importance, as they seem to provide an excellent example of the basic fronts now so favoured in some petrological schools.

The aureole of contact-metamorphosed rocks is well displayed along the northern border of the Insch Mass. The sedimentary rocks were largely shales and slates, but the writer's work elsewhere in the Insch area shows that impure calcareous rocks may form a more important ingredient of the Macduff Group than hitherto recognized. The contact-metamorphism of the argillites has given rise to two zones of products :—

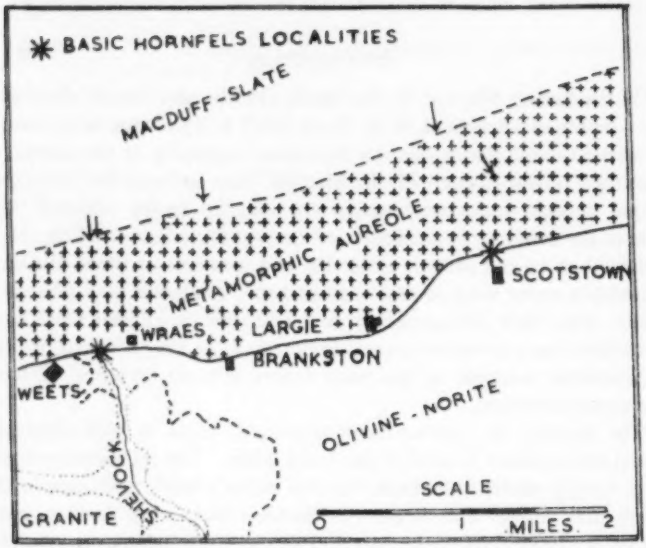
(1) An outer zone of spotted slates in which the slaty groundmass of the original rock persists, the spots being due to new cordierite and andalusite ;

(2) An inner zone of completely recrystallized andalusite-cordierite-hornfels.

The igneous rock at the contacts to be discussed here is an olivine-norite composed of labradorite, olivine, and hypersthene, with less common augite and scarce hornblende, biotite, iron-ore, and apatite. The published analysis of this rock (Read, 1923, p. 124) is given in Table 1.

Field Occurrence of the Basic Hornfelses

In the Shevock Burn, at a locality 270 yards below the road-bridge and 210 yards N.E. of the farmhouse of Weets, is a small crag consisting for the most part of massive blue fine-grained rocks. In the field the different varieties of rocks making the crag are difficult to distinguish from one another, but on slicing they are found to be olivine- and pyroxene-bearing hornfelses intimately associated with quartz-biotite-plagioclase-hypersthene-hornfelses of undoubted sedimentary origin. A few yards farther downstream and on the same bank there

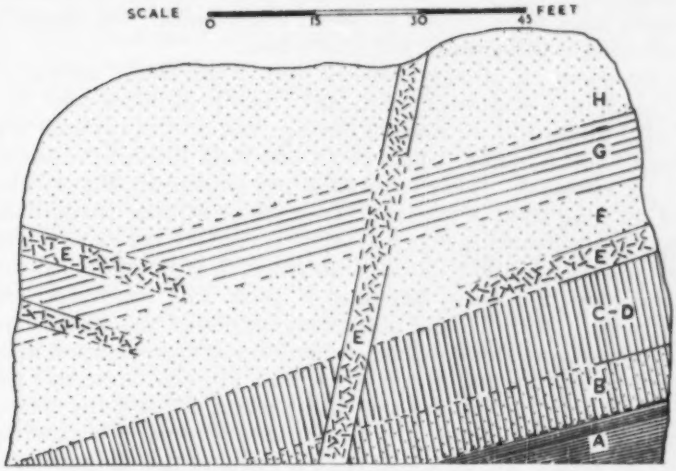


Text-fig. 1.—Sketch map showing the occurrence of the olivine-bearing contact-rocks.

are found numerous outcrops of coarse spheroidally-weathering olivine-norite. It is reasonable to believe that the fine-grained basic hornfelses are at or near the junction of the olivine-norite and the Macduff Slates. The occurrence and field relationship of these rocks are shown in Text-fig. 1.

From the Shevock Burn the margin of the olivine-norite can be followed eastwards by Brankston and Largie to Scotstown. In a small quarry 280 yards N.W. of Scotstown there are well displayed fine-grained basic hornfelses similar to those of the Shevock Burn. Slightly farther northwards from the quarry cordierite-andalusite-hornfelses normal to the Insch aureole are found. In the Scotstown quarry the

relationships are those shown in Text-fig. 2: olivine-norite on the south is in contact with olivine-bearing hornfelses which form a band between the igneous rock and the clearly sedimentary hornfelses to the north. The contact-rocks of the quarry are again difficult to distinguish in hand specimen and the details given in Text-fig. 2 are based upon microscopic examination. It is established that the olivine-bearing hornfelses are present at the border of the olivine-norite to a width of about 20 feet. In the Shevock Burn the rocks are so badly exposed that it is difficult to make out the width of the basic types—possibly it is the



TEXT-FIG. 2.—Plan of the Scotstown Quarry showing the occurrence of the Olivine-Norite and the Country Rocks.

(A) Olivine-Norite; (B) Olivine-bearing Hornfels, banded and garnetiferous; (C–D) Olivine-bearing hornfels; (E) Granite; (F) Quartz-cordierite-biotite-spinel-plagioclase-hornfels; (G) Cordierite-spinel-plagioclase-hypersthene-hornfels; (H) Quartz-cordierite-biotite-spinel-plagioclase-hornfels.

same as in the Scotstown quarry. Both localities of the basic hornfelses lie, therefore, at the immediate margin of the olivine-norite, against the Macduff Slates (Text-fig. 1).

PETROGRAPHY

The rock types met with at the contact of the olivine-norite are olivine-bearing hornfels, pyroxene-hornfels, quartz-biotite-plagioclase-hypersthene-hornfels, and quartz-cordierite-biotite-spinel-plagioclase-hypersthene-hornfels. The olivine- and pyroxene-bearing hornfelses are described first.

OLIVINE-HORNFELS.—In the hand-specimen the olivine-hornfels is

a dark blue-black rock, fine-grained and compact. The only megascopically recognizable mineral is biotite, which forms small glistening flakes. The rocks are hard and tough rather than splintery. In thin slices (see Figs. A-E, Plate VIII), they are seen to be composed mainly of the ferromagnesian minerals—augite, hornblende, biotite, olivine, and iron-ore, in order of abundance. Basic plagioclase felspar is the only colourless component. Hypersthene and garnet are very rare.

Augite, which is the chief component, occurs usually in small equidimensional grains, often collected into trains and sometimes coalescing to form small sieved prisms. Less commonly the pyroxene builds larger prisms located in definite coarse bands. The mineral is colourless or pale greenish, and is quite fresh.

Hornblende is strongly coloured with a pleochroic scheme of x = yellow, y = chestnut brown, and z near y. It forms stout subhedral prisms with an excellent cleavage, and is often collected into hornblendic layers which have only a little black iron-ore in addition to the amphibole. *Biotite* forms either very large poikiloblastic plates or else small plates of the normal grain size of the other components. It is usually pleochroic from pale yellow to deep rich red—a vivid type of the usual " hornfels " biotite pleochroism. It is plentiful in most of these rocks and occurs associated in bands with hornblende and pyroxene. *Olivine* is quite a common component and forms large shapeless masses often completely or partly altered to green serpentinous products with the liberation of much black iron-ore. Sometimes the olivine occurs in ramifying and wormy shapes imitating the ophitic texture of igneous rocks. Occasionally it is bordered by hornblende against felspar. The optic axial angle is nearly 90° and the mineral is negative ; this indicates a FeO content of over 12 per cent. *Iron-ore* forms very abundant small scattered grains or large poikiloblastic masses. *Plagioclase felspar* is the only colourless component and is in quite subordinate amount. It usually forms fresh clear grains, often untwinned but occasionally showing repeated twinning. In some rocks it occurs as large plates poikilitically enclosing other components or else it forms euhedral crystals containing drops of pyroxene and olivine. The felspar is a bytownite, negative in sign with an extinction angle (in section $\perp Z$) $X'\wedge 001 = -45°$, indicating a composition $Ab_{29}An_{71}$. Hypersthene and garnet were noted in a slightly banded olivine-bearing contact rock. *Hypersthene* is granular, vividly pleochroic, with x = rose pink, y = yellow, and z = bluish green, and occurs in granular aggregates and bands. *Garnet* is pink in colour, granular, and is found in a band associated with granular *iron-ore* (Fig. D, Plate VIII).

The characteristic texture of this group of rocks may be called poikiloblastic-granular. All the minerals occur in small equidimensional

grains, but often these grains coalesce into sieved plates enclosing small granules of the other components. This sieve texture is excellently developed by biotite and hornblende which enclose small rounded grains of pyroxene or felspar. Akin to the sieve texture is what may be termed drop-texture ; this is perhaps best shown by the euhedral plagioclases, seen in certain slices, that enclose numerous drops of pyroxene and olivine. Of the pairs of minerals taking part in these poikiloblastic associations we may give : felspar in olivine, iron-ore in olivine, hornblende in olivine, pyroxene in biotite, felspar in biotite, olivine in plagioclase, and pyroxene in plagioclase. In some slices, these rocks, by the euhedrism of the felspars and the poikiloblastic habit of the mafic constituents, show a texture approaching the doleritic, but even here every component is sieved (Fig. E, Plate VIII).

Besides exhibiting these textures the basic hornfelses are often banded with narrow bands of different mineralogical composition. Bands rich in pyroxene or hornblende or biotite or olivine alternate. Another type of banding is shown by the incoming of the coarse pyroxene-bands already noted. There is seen in every slice a certain amount of patchiness in the disposition of the several components.

A chemical analysis of an olivine-bearing hornfels from the Shevock Burn is given with others in Table 1. The analysis of the Shevock Burn rock agrees with its mineralogy. In the Norm Classification it falls into Class 4, Order 2, Rang 2, Subrang 3 ; this Subrang in Washington's Tables (1917) is occupied by three analysed rocks, a hornblendite from Colorado, a secretion in granite from the Black Forest, and a pyroxenite from Monzoni. The Shevock Burn rock differs from the usual igneous rocks of similar silica percentage in the calc-alkaline series in its relative poverty in magnesia and richness in lime. Analyses of the associated rocks, the Insch olivine-norite, a slate, and a calcareous hornfels from the Macduff Series, are given for comparison in Table 1.

Pyroxene-hornfels.—Associated with the olivine-hornfels in the Shevock Burn is a pyroxene-hornfels of considerable interest. The rock is a dark fine-grained granular hornfels, seen in slice to be composed dominantly of pyroxene with scarce plagioclase felspar, apatite, and pyrite. The pyroxene forms anhedral grains, slightly altered to amphibole, and is vividly pleochroic with x = lilac, $y = x$, and z = greenish yellow ; the mineral is optically positive and it is a *titaniferous augite* (Fig. F, Plate VIII). Plagioclase occurs in the interstices of the pyroxene and is cloudy and altered. Pyrite occurs as granules and clots, apatite forms prisms and needles.

This striking and unusual pyroxene may be compared with the very similar species investigated by Dixon and Kennedy (1933, p. 112) from a xenolith collected by Read (1935, p. 623) from the gabbro of the Haddo House Mass in Eastern Aberdeenshire. It recalls, too, that of

TABLE I.—CHEMICAL COMPOSITION

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SiO_2	42·95	44·65	58·47	51·88	47·45	48·20	58·04
TiO_2	3·20	3·69	1·30	1·14	1·42	0·65	0·71
Al_2O_3	10·49	17·95	18·27	16·24	20·92	19·53	18·66
Fe_2O_3	1·73	1·09	1·66	0·93	2·87	tr	1·51
FeO	12·33	15·02	6·49	8·39	6·24	10·60	5·98
MnO	0·42	0·36	0·25	0·19	0·02	0·14	0·08
(Co, Ni)O	nt. fd.	nt. fd.	tr	—	tr	—	—
BaO	—	0·01	0·05	—	—	—	—
CaO	14·00	9·50	0·78	7·96	9·85	8·51	1·02
MgO	11·44	3·34	2·64	6·84	5·70	9·28	3·24
K_2O	1·76	0·54	2·84	2·64	2·89	0·32	3·28
Na_2O	0·43	2·79	2·46	2·76	2·14	2·52	2·12
H_2O	0·60	0·55	3·46	0·78	0·92	0·73	3·66
P_2O_5	0·43	0·09	0·15	0·07	0·29	0·19	0·12
FeS_2	nt. fd.	0·12	0·60	—	—	—	—
Fe_5S_7	—	0·12	0·72	—	—	—	—
CO_2	—	0·28	0·08	—	—	0·02	0·14
S	0·37	—	—	—	—	0·03	0·17
SO	tr	—	—	—	—	—	0·70
Misc	—	—	—	—	—	—	1·13
	100·15	100·10	100·22	99·82	100·71	100·72	100·56

(1) Olivine-bearing hornfels, Shevock Burn, 270 yards below bridge, Insch. Anal. W. H. Herdsman.
(2) Olivine-norite, old " sand " pit, Bonnyton, Insch. Anal. E. G. Radley. H. H. Read, *Mem. Geol. Surv. Scot.*, 86 and 96.
(3) Macduff Slate, Macduff, Banffshire. Anal. E. G. Radley. H. H. Read, *Mem. Geol. Surv. Scot.*, 86 and 96.
(4) Impure calcareous hornfels xenolith in diorite, 210 yards west of north of Craig Hall, west of Kennethmont, Insch. Anal. W. H. Herdsman.
(5) Hornfels inclusion in gabbro, Jordon Lake, near contact-altered Knife Slate. Anal. W. S. Yarwood. F. F. Grout, *Bull. Geol. Soc. of America*, 1930.
(6) Olivine-gabbro. Duluth. Anal. F. F. Grout. F. F. Grout, *Bull. Geol. Soc. of America*, 1930.
(7) Virginia Slate. Duluth. Anal. George Ward. F. F. Grout, *Bull. Geol. Soc. of America*, 1930.

the pyroxene-hornfelses described by Coulson (1930, p. 448) from the contact of a calcareous sediment and a gabbro plug in Rajputana. There may be a significance in this association of titaniferous-augite contact-rocks and gabbros in these three localities.

Associated Hornfelses.—The discrimination between the various types of hornfels in the contact zone is, as has been mentioned, a difficult matter in the field. Possibly a more thorough slicing would lead to the determination of a larger number of varieties and especially of clearly sedimentary types. The broad association of types in the Scotstown quarry given in Text-fig. 2 would doubtless be elaborated by further work. Among the varieties investigated are the following :—

(i) Quartz-biotite-plagioclase-hypersthene-hornfels.—This rock comes from the Shevock Burn and occurs in the same small crag as the olivine-bearing rocks already described. It is a pale yellowish-grey siliceous hornfels containing predominantly granular quartz. In subordinate quantities occur tattered biotite split by a colourless mineral of medium relief, possibly prehnite, tabular acid andesine felspar, granular iron-ore, and scarce altered hypersthene. The rock is a recrystallized impure grit.

(ii) Quartz-cordierite-biotite-spinel-plagioclase-hornfels. This rock forms the band next to the olivine-bearing hornfels in the Scotstown quarry. It is a grey siliceous rock showing in slice abundant granular quartz, pinitized cordierite, yellow to red pleochroic biotite, and irregular andesine felspar ; scarce deep green spinel is associated with iron-ore grains, sillimanite is occasionally seen, and garnet is locally present. The rock may be classed with the cordierite-plagioclase hornfels of Class 3 of Tilley's shale-calcic group.

(iii) Cordierite-spinel-plagioclase-hypersthene-hornfels. Adjacent to the quartz-cordierite-biotite-spinel-plagioclase-hornfels in the Scotstown quarry there occurs a dark greenish grey fine-grained hornfels showing in slice cordierite, spinel, plagioclase, hypersthene, biotite, and iron-ore. Cordierite is granular and pinitized and contains spinel as inclusions. Hypersthene is granular and pleochroic and occurs in bands. Brick-red biotite is found as irregular plates associated with the hypersthene and iron-ore. Spinel is green in colour, granular, and fairly abundant. Plagioclase felspar is an andesine, tabular in shape, slightly altered, and contains spinel as inclusions. The rock is a silica-poor hornfels belonging to Class 4*a* of Tilley's (1924) shale calcic group. Some slices show scarce orthoclase, quartz, and pyrite.

Origin of the Basic Hornfelses

The basic hornfelses just described are intimately associated with the Macduff Slates of the contact in their structure, texture, and field relationships, and it is difficult to separate the olivine-bearing types from the rest of the hornfelses, some of which are clearly original sedimentary rocks. On the other hand, the olivine-bearing types differ in structure and texture from the adjoining olivine-norite. The basic hornfelses are obviously the result of contact metamorphism and it may now be considered what was the nature of the original rock upon which this metamorphism has acted. There are at least three possibilities.

(*a*) The basic hornfelses were originally country-rocks of the Macduff Slate group and have retained their composition unchanged.

(*b*) The basic hornfelses represent the chilled edge of the olivine-norite or of another variety of Insch gabbro or earlier basic igneous rock which has been contact-metamorphosed by a later surge of basic magma.

(*c*) The basic hornfelses were originally members of the Macduff Slate group and attained their present composition by the transfer of lime, ferrous iron, magnesia, and titania from the Insch basic magma—in modern language, they represent a basic front.

According to the first suggestion, in which no transfer has taken place across the contact, the basic hornfelses may be considered as sedimentary bands intercalated with the remainder of the more normal slates, grits, and impure limestones of the Macduff group. Difficulties in this view are many. Chloritic, lateritic, or bauxitic beds would not give rise to the Shevock and Scotstown rocks, and these are unlike metamorphosed basic detritus in their mineralogical characters ; moreover, such beds have not yet been discovered in the great development of the Macduff Group in North-east Scotland. The basic hornfelses might, however, be considered as extreme types of calc-silicate hornfelses. In Table 1 there is included an analysis of an impure calcareous rock of the composition diopside-tremolite-spinel-hornfels occurring as a xenolith in the Insch granite a mile or so to the west of the Shevock Burn. The olivine-bearing hornfels is richer in lime, magnesia, and iron-oxide. It appears reasonable to the writer to consider that the basic hornfelses do not represent any sedimentary rock that has kept its original composition unchanged on metamorphism.

According to the second suggestion, the basic hornfelses, especially the olivine-bearing types, represent the chilled edge of an Insch gabbro or a relic of older basic rock thermally metamorphosed by the later

influx of the olivine-norite magma. Detailed comparison shows, however, that the composition of the olivine-bearing hornfels differs from that of any of the abundant basic rocks of the North-east Scotland petrographic province, nor is it like any of the older igneous rocks of the same region. The basic hornfelses are banded, a feature unusual in chilled margins, and they are intercalated with obviously sedimentary types. These basic hornfelses, therefore, appear unlikely to be of original igneous parentage.

The third proposal suggests that the basic hornfelses are Macduff sedimentary rocks modified in composition by the influx of material from the gabbro magma. This suggestion agrees with their geological association with the cordierite-bearing hornfelses, their textural and structural characters, and their peculiar chemical composition. The Macduff Group is mainly of argillaceous composition but as we have seen contains also members richer in lime. From whatever type the olivine-bearing hornfelses may have been derived they are certainly very basic in composition and can have been produced from any Macduff rock only by an influx of titania, ferrous iron, lime, magnesia, manganese, and phosphorus. The process is one of basification connected with the basic magma.

The occurrence at Haddo House and in Rajputana under comparable circumstances of hornfelses similar to certain varieties of the basic hornfelses of Insch has been mentioned in connection with the presence of titaniferous augite in a Shevock Burn rock. But the closest resemblance to the Insch basic group as a whole is provided by rocks described from the great Duluth gabbro lopolith by Grout (1930). Here, basic hornfels xenoliths are abundant in the lower part of the gabbro body ; they have a hornfels-texture and show the same mineral composition as the gabbro itself. Further, the slate in contact with the gabbro has been modified for a distance rarely exceeding ½ mile into a rock also resembling the gabbro in composition. Analyses of a basic hornfels enclosed in the Duluth gabbro, together with those of the olivine-gabbro and Virginia Slate of the same occurrence are given in Table 1. Assuming that the parent rock of the inclusion was the Virginia Slate, the enrichment that has taken place in the production of the Duluth hornfels is one mainly of lime and iron-oxide. Grout considers that the slate occurring as xenoliths inside the olivine-gabbro and at its contact has been " gabbroized ".

Conclusion

It appears possible to the present writer that basic hornfels selvages to gabbro bodies may not be so rare as at present seems to be the case. The basic selvage at Insch is produced by an influx of Ca, Mg, Fe, Ti, Mn, and P. It provides a magnificent example of a basic front. In the

discussion that surrounds the general notion of basic fronts it must now be remembered that some basic fronts have also a basic back.

ACKNOWLEDGMENTS

The writer wishes to acknowledge his indebtedness to Professor H. H. Read, of Imperial College, London, for extensive discussion and criticism that have helped in the construction of this account and also for permission to make use of the hitherto unpublished analysis of the olivine-hornfels from the Shevock Burn. The cost of the analysis of the calc-silicate hornfels, together with those of other rocks of the Insch mass to be dealt with later, was defrayed by a grant from the Government Grant-in-Aid of Scientific Investigations administered by the Council of the Royal Society.

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EXPLANATION OF PLATE VIII

Microphotographs of Olivine-hornfelses (A–E) and Pyroxene-hornfels (F)

A. Biotite, pyroxene and olivine, showing sieve-structure, × 7.

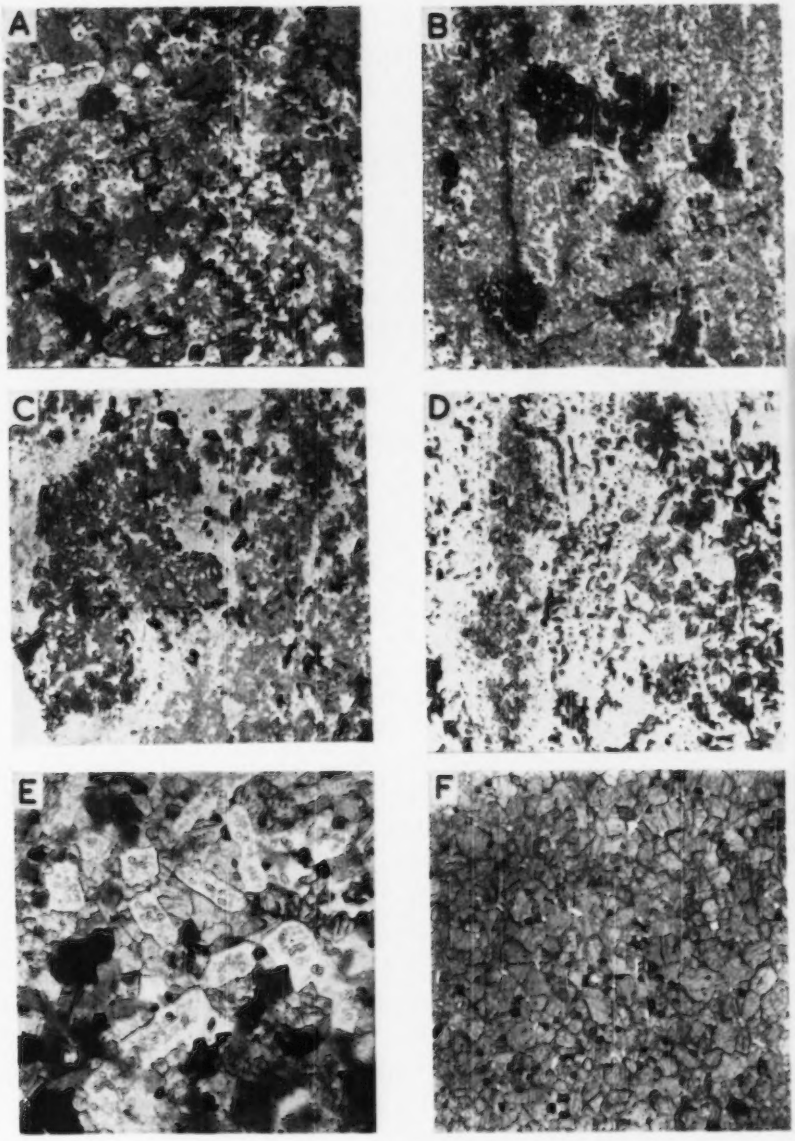
B. Olivine porphyroblasts, rimmed by serpentine and iron-ore, in a groundmass of plagioclase filled with granular pyroxene, × 7.

C. Large porphyroblastic olivines, sieved by felspar, on left; on right and centre, granular pyroxene in felspar, × 7.

D. Bands, from left to right, of pyroxene-felspar; garnet; pyroxene-felspar; olivine-felspar-pyroxene, × 7.

E. Lath-shaped felspar, sieved by pyroxene and olivine, between larger grains of pyroxene and olivine, giving a kind of ophitic texture, × 27.

F. Granular titaniferous pyroxene with scarce altered felspar and granular iron-ore, × 7.



Photographs by Mr. J. A. Gee.

Basic hornfelses from the Insch Mass, Aberdeenshire.

The Tectonics of Holland Arms

By Edward Greenly

Abstract

A distinction is drawn between the gneisses above the Craig, excessively shattered, and those of the Gaerwen country, now regarded as autochtonous and on a different tectonic horizon.

Ever since the classic researches of Callaway, of some sixty years ago, this has been a special district for metamorphic studies, at any rate of mica-schists, comparable with that of Scourie in regard to hornblende-schists. Nothing, however, has been done to elucidate its tectonics. Even the great Callaway, intent as was natural upon the acid rocks, made no distinction between basic rock and basic rock : all in his eyes were just simply " the diorite ". We have learned to distinguish hornblende-schist from hornblende-gneiss ; we must now take a further step, and distinguish gneiss from gneiss.

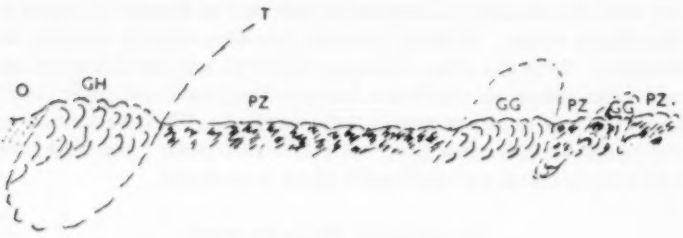
The Gneiss of Holland Arms

The most conspicuous formation is the gneiss of the heights above the Craig which, as it extends to the road at the Inn, may be alluded to as the Gneiss of Holland Arms. It is excessively shattered and mylonized, and cannot possibly have come up from underneath the mica-schist. Yet, if we bring it simply down from above, it would be locally brecciated, it would not be shattered as it is. It must have had a considerable journey. But, from what is known in the N.W. Highlands, even that might not be enough. If, however, we look at the form of its outcrop we see that this is not equi-dimensional, as is usual with outliers from a horizontal thrust-plane ; it is long and oval, as if in a synclinal infold. Now we know that the thrusts of the recumbent order are very apt to be refolded. So, if we suppose that as well as a journey of several miles it has been very tightly nipped, that will account for its condition.

The Gneisses of Gaerwen

At and to the north of Gaerwen there are several tracts of gneiss which hitherto I have correlated with the gneiss of Holland Arms. This I now find to be an error. There are a number of reasons. (1) My suspicions were first aroused by noticing on the 1-in. map that the trend of most of them differed by a good many degrees from the trend at Holland Arms. (2) They are nothing like as shattered, in fact they are in quite good condition. (3) They are decidedly more basic, not to mention that at Holland Arms there is a wedge of acid gneiss with garnet and some sillimanite : facies is not the same, a feature which

confirms the view that the gneiss of Holland Arms has come from several miles away. (4) Where adjacent to mica-schist (I do not here consider faults which may be Palaeozoic or even later) no signs of rupture have been detected, whereas the gneiss of Holland Arms is riding on a powerful thrust-plane. (5) They are affected by the P.Z. metamorphism, which the gneiss of Holland Arms never shows the least sign of being. They are, it is manifest, rising from below. But the P.Z. mica-schist is a condition of Fydlyn Beds, and we have recently found that the Fydlyn Beds are at the base of the Bedded Succession.



PZ = The Mica-schist.
GH = Gneiss of Holland Arms.
GG = Gneiss of Gaerwen.
T = Folded Thrust plans of Holland Arms.
O = Ordovician.
Scale (horizontal) about 8 inches to 1 ml.
Scale (vertical) exaggerated.

TEXT-FIG 1.

Thus, these gneisses are in their true stratigraphic position. They must be rising on anticlines, upon unruptured anticlines.

RELATIONS

The gneiss of Holland Arms is taken in upon a syncline. Is it credible that in the space of half a mile one and the same formation should here be taken in on a syncline, and there be rising up on an anticline ? That, however, would be possible if the outcrops were upon different tectonic horizons. And such is here the case. The gneiss of Holland Arms is riding on a thrust-plane, a folded thrust-plane of the recumbent order ; the gneisses of Gaerwen are autochthonous.

So we are able to draw a section. Doubtless it is greatly generalized and will be much improved upon by our successors. But then, it is a pioneer.

Note on the Sandstone Injections of Eathie Haven, Cromarty

By Charles D. Waterston

Abstract

The sandstone injections which penetrate the Kimmeridgian strata of Eathie are described, the presence of sill-like injections being particularly noted. It is considered that the occurrence of "sandstone-sills" at Eathie is of importance in ascertaining the mode of origin of the injections of the locality. It is further suggested that the sills support the hypothesis that the injections are the result of infilling of fissures from below by sand and water.

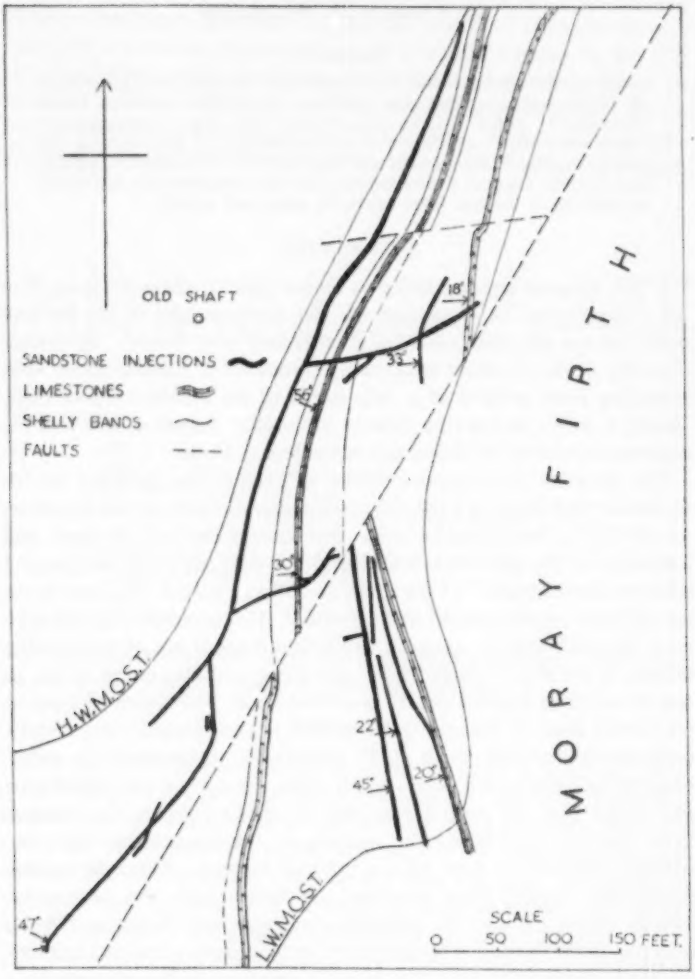
Introduction

THE Kimmeridgian outcrop at Eathie Haven—three miles south of Cromarty, in Ross-shire—is the southernmost of the Jurassic outcrops on the east coast of Sutherland and Ross. At Eathie (Text-fig. 1) the Jurassic rocks are exposed in a narrow shore strip extending from a third of a mile north of the Haven to Bow Buoy Skerry, a rocky eminence a mile to the south. Except at low tide the exposure is covered by the waters of the Moray Firth.

The presence of sandstone dykes at Eathie was recorded by Sir Roderick Murchison in 1827, the first description of the dykes, however, was by H. E. Strickland in 1840. Strickland described the form and substance of the dykes and mentioned "two dykes which are parallel with the stratification" of the shale, as well as "others which are in no part of their course parallel to the strata". He considered the dykes to have been infillings by aqueous deposition from above of pre-existing fissures in the shale. Hugh Miller gave some attention to the dykes, in one of which he found a poorly preserved fossil. He described them in his *Sketch Book of Popular Geology* (1859), and accepted Strickland's interpretation of their origin. J. W. Judd (1873) summarized the earlier observations and added others of his own. He agreed with Strickland and Miller that the fissures had been filled from above, but differed from them in considering the opening of the fissures and the concomitant infilling to have been a gradual process. Although several subsequent workers have described the Eathie beds, the next to be directly concerned with the sandstone injections were Bailey and Weir, who visited the locality in connection with their study of related features in East Sutherland (1932). The present investigation is supplementary to the work of Bailey and Weir, being a further inquiry into one aspect of their more comprehensive study.

The Kimmeridgian rocks are richly fossiliferous grey and black shales with bands of limy nodules; three bedded bituminous limestones occur in the succession. The three basal Kimmeridgian ammonite

zones (Spath, 1935, p. 74) are represented at Eathie, though only parts of the zone of *Pictonia baylei* and *Rasenia cymodoce*, and of the zone of



TEXT-FIG. 1.—Sketch map of the central part of the Kimmeridgian outcrop at Eathie showing the sandstone injections.

Pararasenia mutabilis, are present. The Jurassic rocks have been down-faulted against Moine Gneiss and Middle Old Red Sandstone, with both of which they are in contact to the west. The strata lie within the

zone of the Great Glen Fault and are therefore very much disrupted. Although the main wrench movements of the fault ended before the Upper Carboniferous (Kennedy, 1946), subsequent movement of the Kimmeridgian beds against the pre-Mesozoic strata has resulted in a normal fault along the same line of weakness. The Kimmeridgian beds are cut by small faults which trend in two directions (Text-fig. 1). The middle part of the exposure is disturbed by a fault running obliquely across the shore at about 30° east of north, having a downthrow on the seaward side of about 30 feet. A parallel fault zone may be present further north. Minor faults, most of them too small to map, make up a second group, and cross the strata at about 75° east of north.

The strike of the Jurassic beds is in the form of an arc running practically north and south in the southern part of the exposure, but changing towards the north to a north-easterly trend. The general dip is 20° east to south-east. The large boundary fault, however, has contorted the beds at their contact with the pre-Mesozoic rocks, the dip gradually increasing towards the fault until the strata are vertical at the contact.

Field Relations

The injections were mapped on a scale of 1 inch : 50 feet. It was found that the injections could be classified into three types—cross-cutting, strike, and vein injections.

The cross-cutting injections are dyke-like bodies of the classic "Kintradwell type" (Murchison, 1827) striking across the strata at an angle of some 75° east of north. They vary in thickness from mere stringers of 1 or 2 inches across to a 3-ft. dyke, which is the widest injection of this type at Eathie. They may be as much as 150 feet long, but many minor changes of trend occur, and normally they form a complex of ramifying branches.

The strike injections are developed on a much larger scale than the cross-cutting type. The usual thickness of the strike injections is 3 to 5 feet, and lengths of 100 yards are not exceptional, one being over 200 yards long. Injections of this type are not subject to the sudden changes in strike or to the repeated branching seen in the discordant injections, but follow the strike of the shale with little or no divergence. Occasional branching is seen locally, but in no case is it of the complex type characteristic of the cross-cutting injections. Not only do the strike injections conform to the strike of the beds, but in large measure they conform to the dip also. Strike injections penetrating the vertical shales near the boundary fault have a dyke-like attitude: those occurring in the gently dipping strata near low-water mark, however, are in the form of gently dipping concordant sheets which, as they

simulate the habit of intrusive igneous sills, should be termed " sandstone sills ". The lithology of the sills (see below) indicates that they are not interbedded sandstones. In one case the field relationships alone are sufficient to prove the sill to be a subsequent injection. The intrusion is in the form of a transgressive sill, which is seen in strike section. It follows one horizon in the shale for 100 feet and during the next 30 feet it transgresses into higher zones of the shale until it reaches the base of a bedded limestone with which it remains in contact as far as it can be traced. Where transgressing the shales its dip is 29°, but in contact with the limestone the dip is only 20°. The association of a sandstone sill with the base of a bedded limestone has been found in three of the occurrences at Eathie. Where there is a notable change in strike both discordant and concordant characters may be combined in a single injection.

In addition to the main injections there are subsidiary sandstone veins too small to be shown on the map. In the limestones, in which they form " net-veining ", these veins are especially abundant and complex.

Slickensides are sometimes preserved on the exposed walls of upstanding dykes. The cast of the bedding planes of the shale with which the dyke has been in contact can also be seen. The junction between the injections and the Kimmeridgian strata is extremely sharp, the shale showing no contortion due to the emplacement of the injection. The sandstone dykes and sills do not cross the boundary fault which separates the Jurassic from the pre-Mesozoic rocks, but are found only in the Kimmeridgian.

Lithology

The sandstone is hard and massive, free from bedding planes, and interrupted only by a series of transverse joints. Throughout dykes and sills alike the texture remains constant. Fracture faces of the sandstone exhibit lustre mottling. In some cases, particularly among the sills, bands of shale fragments near the edges of the injection run parallel to each wall.

Thin-sections were made from five of the hand specimens collected from the dykes and sills. These show that clear quartz grains, many with undulose extinction, are the most abundant sand fragments. Orthoclase, microcline, microperthite, and labradorite are fairly common, and occasional flakes of muscovite and biotite also occur. Heavy minerals, though not common, include colourless zircon, tourmaline, and black iron ores. The sand fragments, which are equidimensional and sub-angular, appear as isolated grains set in a calcite cement. The cement occupies 43 per cent of the sandstone by volume, and consists of large anhedral calcite crystals, each of which

encloses many sand grains. The grains appear to have acted as nuclei for the growth of the cement, which has later recrystallized. In most cases the contact between the grains and the cement is sharp, but some of the quartz grains have been replaced at their edges by calcite. The mineral content and texture of the sills appear to be similar to those of the dykes. That some of the injections have been subjected to pressure after consolidation is shown by the occurrence of cracked quartz and feldspar grains, and by the development of glide planes along the rhombic cleavage of the calcite cement.

Age and Origin

There does not appear to be enough field evidence at Eathie to enable the observer to date with any accuracy the time of injection of the sand into the Kimmeridgian strata. That the shale had already consolidated before the injection of the sand is evident, since the latter occupies fissures in the shale, consolidated fragments of which are incorporated in the sandstone as inclusions. Casts of bedding planes of the shale, seen on the walls of some of the dykes, show the same dip as do the shales to-day, an observation that might indicate that the dykes were injected after the shale had been upturned. This assumes either that the sand consolidated without further movement of the shale or that, if there had been any subsequent movement, the consolidated sandstone and shale moved as a unit ; it ignores the possibility that movement of the shale might have occurred after injection had taken place, but while the sand was still mobile. Additional field evidence, e.g. the occurrence of boulder-beds in East Sutherland, led Bailey and Weir (1932) to suggest that the fissuring of the Lower Kimmeridgian shales of that area was of seismic origin, due to movements of the Ord Fault in Kimmeridgian times. This interpretation may be brought into relation with the injection phenomena of Eathie, the origin of which may well have been contemporaneous with the East Sutherland occurrences. It seems probable that the fissures at Eathie, into which the sand penetrated, were also of seismic origin as a result of dislocations along the Great Glen Fault.

It is apparent from the parallel alignment of the flaky minerals with the walls of the injections and the presence of bands of shale inclusions near the edges of the dykes and sills that the sand has flowed into its present position. Since the injections are exposed only on a wave cut platform, however, their vertical direction of thinning cannot be seen. Two hypotheses may therefore be put forward regarding the mechanism by which the sand was injected into the fissures in the shale : the first depending solely on the action of gravity on the sand forming the sea floor at the time of fissuring, the second on the mobility of sand and water or swimming-sand, which is akin to that possessed by an igneous

rock melt. The gravitational hypothesis, supported by all previous workers on the sandstone injections of Eathie, accounts for most of the field evidence connected with the dyke-like injections. That this was the mode of origin of certain sandstone dykes in the Helmsdale district has been proved by Bailey and Weir (1932, p. 452), where a dyke of shelly sandstone is in direct continuity with an overlying bed of like material. The source of the injected sand, however, is not exposed at Eathie. The gravitational hypothesis is not satisfactory in its application to the origin of the sills. Long tensional fissures, having a very low angle of dip, would have to be produced and maintained for a sufficient period to allow sand to flow into them and consolidate there. It seems most unlikely that the strength and elasticity of the rocks would be sufficient for such fissures to form and be maintained on such a scale so near the surface.

Sand occurring on the sea floor in Kimmeridgian times is not the only source from which the injected material could have been derived, since under certain conditions it may have originated in layers interbedded with the shales through which the injections now pass (P. Pruvost, 1943) or in a sandy horizon below them. The conditions required are that the sand should be potentially mobile and that sufficient pressure should be exerted on it to force it up any lines of weakness which are present in the strata above. It is possible that the source bed may be rendered momentarily mobile by thixotropic change from a solid to a mobile condition, due to seismic shock (H. V. Dunnington, 1945, P. G. H. Boswell, 1948). That such favourable conditions are commonly produced during earthquakes is suggested by the development of sand vents and fissures (Oldham, 1899, p. 87 ; Shrock, 1949, p. 214). The strongest evidence at Eathie that the sand was injected into the strata from beneath is the occurrence of sandstone sills, particularly the transgressive sill already described (p. 136). In the latter case it would appear that the incoming sand travelled up the bedding planes of the shale, transgressing into higher strata until the base of the bedded limestone was reached. Since the injected material could not breach this strong impervious bed, it was confined to a level between the base of the limestone and the shale, except where small cracks in the limestone permitted vein-like injections to penetrate it. The lithological similarity between the dyke-like and sill-like injections would suggest that the material of both types came from the same source, and that the mechanism of injection was probably the same in each case, namely the intrusion from below of sand and water into fissures of seismic origin in the Kimmeridgian strata.

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CORRESPONDENCE

INVESTIGATION OF THE TORRIDONIAN

SIR,—In view of Professor Read's recent plea for a reinvestigation of the Torridonian (Abstract Proc. Geol. Soc. London, No. 1459, 17th February, 1950) it is perhaps desirable to make clear that the writers have already been engaged in this work for some two years. It is proposed to communicate the results in a series of papers of which the first are in an advanced state of preparation.

DEPARTMENT OF GEOLOGY,
THE UNIVERSITY, LEEDS, 2.
20th February, 1950.

W. Q. KENNEDY.
J. E. HEMINGWAY.

KENT'S CAVERN, TORQUAY

SIR,—Since digging started in Kent's Cavern in about 1824, large quantities of archaeological and palaeontological material have been unearthed, but nowhere does there appear to be any complete record of the places in which this material is now deposited.

At the request of a Committee of Section H of the British Association for the Advancement of Science I am therefore now endeavouring to complete a detailed register.

If any of your readers should know of such material in private collections or in "forgotten" drawers and cupboards in museums and other public institutions, I should be most interested to know; the archaeological items include objects of the Bronze Age, Early Iron Age, and Roman period, as well as representatives of nearly all the palaeolithic industries.

I should also be very glad to have views as to the nature of the cause of the difference between the granular and the crystalline stalagmite in Kent's Cavern, and opinions as to how it may have been possible for fragments of the lower stalagmite to be broken off and included in the upper *without* the action of a stream (of whose hypothecal existence at any time there seems scanty evidence).

BARBICAN HOUSE MUSEUM,
LEWES.
21st February, 1950.

EDWARD PYDDOKE.

PRE-CAMBRIAN FORMATIONS OF INDIA

SIR,—The paper by Professor Holmes on "The Age of Uraninite and Monazite from the Post-Delhi Pegmatites of Rajputana" (*Geol. Mag.*, lxxxvi, 1949) is of much interest to all students of the pre-Cambrian formations of India. The wide areas covered by these formations in Peninsular India, the facts that there are several systems of such rocks, and that the continuity of exposure at the surface is interrupted by wide spreads of younger formations such as the Gondwanas and the Deccan Trap, make the correlation of the various separate exposures of these older formations a matter of much difficulty, a difficulty that is accentuated by the intensity of the metamorphism to which they have been subjected appearing to vary from one part of India to another.

Some years ago I began an attack upon this problem in a memoir entitled "An Attempt at the Correlation of the Ancient Schistose Formations of Peninsular India" (*Mem. Geol. Surv. Ind.*, lxx, Part 1 (1936); Part 2, No. 1 (1936); and Part 2, No. 2 (1940)). The method adopted was to divide India into provinces for purpose of discussion, the arrangement proposed being given on page 42 of the work cited, where names are given to fifteen Archaean provinces for Peninsular India, with two provinces for Himalayan occurrences, and one for Burma, making eighteen provinces in all. The boundaries of these provinces are shown on an accompanying map. The intention was to discuss each of these provinces in turn. Those so far studied are :—

Province No. 1. Dharwar-Mysore-Nellore Province (loc. cit., pp. 53–116).
Province No. 2. Chanda-Bastar Province (loc. cit., pp. 117–156).
Province No. 3. Singhbhum-Orissa Province (loc. cit., pp. 157–217).
Province No. 4. Sausar-Balaghat Province (loc. cit., pp. 219–324).

The intervention of the war and other reasons have prevented the completion of this study, but the task has not been abandoned.

In the Introduction to the Memoir (Part 1, pp. 1–51), I have discussed the criteria to be used in this attempt at correlation. One of them is lead-ratios and helium-ratios (pp. 20–5), and it was hoped that the mention of this would incite students of the radio-active properties of minerals to come to the rescue of the stratigrapher struggling with the correlation of rocks devoid of fossils.

The studies made by Professor Holmes and others of such radio-active minerals as have been found in the Indian Peninsula are, therefore, very welcome.

But there are great difficulties in making use of such methods. The first difficulty is, of course, the degree of accuracy of the methods used in basing age determinations upon measurements of lead and other ratios. However, with the introduction of successive refinements and corrections the students of these methods are becoming increasingly confident of the value to be placed upon such determinations and there appears to be a sufficient degree of agreement between the broad results obtained by different methods to allow one to assume that the ages so determined must be accepted as rough approximations to the truth (usually within say 100 to 200 million years and often much closer).

The second difficulty in India is that the radio-active minerals available for study all come from pegmatites, so that whilst the results show the ages of the pegmatites, they give only the minimum ages of the rocks into which the pegmatites are intrusive. These rocks may be hundreds of millions of years older.

In Rajputana there are at least three systems of ancient stratified rocks, each with its own pegmatites. It is only pegmatites of the type intrusive into the youngest of these formations, namely the Delhi system, that have yielded radio-active minerals; but the examples of these pegmatites that carry the minerals studied do not appear actually to cut the Delhis, but only the older gneisses and Aravallis: these pegmatites are thus post-Delhi only by inference. Assuming, however, that the post-Delhi attribution is correct then the uraninite and monazite derived therefrom discussed by Professor

Holmes show that the Delhis are older than 700 to 865 million years. They also show that the rocks that these pegmatites actually cut, namely the gneisses and the Aravallis respectively, which are much older than the Delhis, are also older than 700 to 865 million years.

Professor Holmes also discusses the ages of the uraninite and monazite obtained from mica-pegmatites near Singar, Gaya district, Bihar, the answer being about 900 million years (830–960). These pegmatites cut schists included in my Narbada-Son-Bihar Province No. 6.

He also draws attention to two recent determinations in India of the apparent age of samarskite from the mica-pegmatites of the Nellore district, Madras, one determination giving 830 million years and the other 1,550 million years, these pegmatites cutting rocks regarded as Dharwarian, though this is not the type area of Foote. As Professor Holmes asks, it is desirable that the analysts responsible for these widely discrepant results should repeat their work.

Meanwhile we can summarize the results of determinations of the age of minerals in Indian mica-pegmatites as follows :—

Rajputana	.	700 to 865 million years.
Gaya, Bihar	.	900 (830–960) million years (or 955 $\pm$ 40).
Nellore	.	830 and 1,550 million years.

Except for the second Nellore result these pegmatite minerals all give results between 700 and 960 million years.

The question for me in my task is whether these results help in the correlation of the ancients schistose formations of Peninsular India. I find it difficult at present to see what conclusion can be drawn from these data except that some of the pegmatites of Peninsular India range in age from 700 to 960 (or 1,550) million years. They give no information concerning the relative ages of the schistose formations of different parts of the Peninsula, nor do they show whether the data relate to pegmatites of more than one age, though this seems not unlikely.

Presumable Professor Holmes recognizes the truth of the last sentence, as he attempts to deduce the relative ages of the various schistose belts of the Peninsula by considering whether the strike of one belt cuts that of another. Thus, he deduces that the strike of the ancient rocks of the Satpura Range cuts across that of the Aravalli Belt, so that the Satpura rocks are the younger. I see no reason why the argument should not be used in the reverse manner to deduce that the Aravalli Belt is the younger, except that this type of criterion appears to have little weight. The strike of the Satpura Range is W.S.W. at its eastern end, curling to westerly as one proceeds westwards. The pre-Cambrian rocks of the Satpuras are then covered by Deccan Trap lavas for some hundreds of miles to the west. The strike direction shown on Professor Holmes' map in Text-fig. 1 marked " Satpura Range " does not represent any strike of the older rocks, as these are here covered by the Deccan Trap : any conclusion drawn from the fact that this black line on Professor Holmes' map is nearly at right angles to the Aravalli strike is therefore irrelevant. Instead, there is nothing, on the evidence available, to prevent one from holding the view that the ancient rocks of the Satpura Belt as they have continued westward beneath the Deccan Trap have also continued with their clockwise change of strike so as to emerge in the west from beneath the Deccan Trap with the Aravalli strike, now N.N.W., as exhibited by the Aravallis of Jhabua State, with their contained gonditic rocks. These latter may well be the western stratigraphical continuation of the gonditic rocks of the Sausar series in the Central Provinces on the eastern side of the Deccan Trap cover.

Professor Holmes also attempts to decide the relative ages of other schistose belts of the Peninsula, discussing specially the Satpura belt, the Eastern Ghats belt, and the Dharwar belt. In so far as he relies on his radio-active data in this discussion, his principal difficulty, though he does seem to know it, is that none of the determinations recorded by him relate to any one of

these three belts. Reference to the map forming Plate I of my Memoir cited above shows that Gaya is not situated on my Satpura belt, being in my Province No. 6 instead of in Province No. 8, which runs from the Central Provinces through the Ranchi plateau on an alignment to the south of the Bihar mica belt, and not as shown on Professor Holmes' map. Further, the Nellore pegmatites do not cut rocks in my Eastern Ghats Province No. 15, or its continuation Province No. 12 near Nellore. Instead they lie in my Nellore-Mysore-Dharwar Province No. 1. Further, the strike of the schists and gneisses of Nellore cut by the pegmatites is N.N.W. to N.W., instead of N.N.E. as assumed by Professor Holmes.[1]

Thus, I think it would be wiser not to attempt to draw conclusions concerning the relative ages of the various belts of schists from a consideration of their general strikes. Instead, the only conclusion that can be drawn from these studies of radio-active minerals is that the gneisses and Aravalli schists into which the Rajputana pegmatites are intruded, the schists of the Bihar mica belt, and the schists and gneisses of the Nellore mica belt are all older than 700 to 960 million years.

As a result of his attempt to discuss the relative ages of various schist belts in India, partly on the basis of their strikes and partly perhaps on the basis of his radio-active data which as shown above do not apply, Professor Holmes writes concerning the relative ages of the Satpura and Eastern Ghats belts and the Dharwars : " In any case the Dharwars are older than both." This still remains to be proved. At present both Dr. Krishnan,[2] to whose views reference is made, and I hold the view that the Dharwars are of an age comparable with that of a portion of the schistose formation of Chota Nagpur and the Central Provinces, both of which lie on or to the south of the Satpura belt. This point has been discussed fully in Part 2, No. 2, containing my accounts of Province Nos. 1, 3, and 4, of the Memoir already cited, and the evidence now produced does not provide any reason for a change of the views there set forth. It may be unwise to use the term " Dharwar " away from its home in Southern India, but it is not wrong to attempt to correlate with the Dharwars the schistose formations of other parts of India. The presumption is that whilst Dharwar sediments were being deposited in Southern India sedimentation may also have been in progress further north. It may be judged from the above that I do not accept the broad sequence in the pre-Cambrian areas of India given by Professor Holmes on page 298 of his paper. His Satpura and Aravalli cycles may well be of the same approximate age. I prefer, however, not to deal with cycles and the relative ages of belts, but with the relative ages of the rock systems that form such belts.

With reference to the rocks of my Eastern Ghats Province it is sufficient perhaps to say that I have not yet discussed this province in the Memoir cited above. The rocks of this province are all in the hypo-metamorphic grade (*Geol. Mag.*, 1927, p. 332) of metamorphism : there are no amphiboles, no chlorite, no epidote, no microcline, etc., over many hundreds of square miles of terrain. Instead : pyroxenes, biotite, garnet, sillimanite, and cryptoperthite ; also charnockitic rocks, khondalites, kodurites, and other queer rocks. The rocks of this province are almost everywhere garnetiferous, and form the terrain the study of which led to my idea of an infra-plutonic zone in the earth's crust (*Rec. Geol. Surv. Ind.*, xliii, p. 43, 1913, and *Geol. Mag.*, 1914, p. 65). The correlation of a terrain of this high grade of metamorphism with schistose terrains of the normal type elsewhere in India presents a problem of major difficulty. But I shall not be surprised if the correlation favoured in the end is of the marbles and manganese-garnet-bearing rocks (kodurites) of the Eastern Ghats province with the marbles and manganese-garnet-bearing rocks (gondites) of the Central Provinces

[1] The schists of Nellore do not belong to Foote's original belts of Dharwars, five in number, but are generally regarded as being probably of Dharwar age. See Fermor, loc. cit., p. 99.

[2] See *Geology of India and Burma*, Table 13, p. 144 (1949).

and of Gangpur in Chota Nagpur, and thus also with the Aravallis of Rajputana, as I adumbrated many years ago.

I agree with Professor Holmes that it would be a pity to perpetuate in India a mistake that would be analogous to the misuse of the term "Laurentian" in North America.[1] Let us hope that we shall succeed in avoiding such in India and that future field work will lead to the discovery of radio-active minerals in the older pegmatites, and will also reveal whether the post-Delhi pegmatites of Rajputana are of the same age as the mica-bearing pegmatites of Bihar and Nellore, or are younger. It would be very useful indeed if studies of radio-active minerals did in the end help the stratigrapher by giving him two datum lines in the ancient schistose formations of Peninsular India. At present I prefer the manganese datum line, as exemplified by gondite (and perhaps kodurite) (see *Mem. G.S.I.*, lxx, p. 17, 1936).

However, I do not wish to close this letter on a critical note, but instead to show the great value of the results obtained by Professor Holmes. The work of Dr. A. M. Heron and his party during many years in Rajputana (summarized in *Trans. Nat. Inst. Sci. Ind.*, i, No. 2, 1935) has shown that the ancient schistose rocks of this terrain can be grouped into a succession of four systems, of which the Delhis are the youngest, and a gneissic complex the oldest. It is a splendid thing to know now that the age of the youngest part of this grand succession of unconformable systems is some 735 million years (using Professor Holmes' preferred value) or nearly 250 million years older than the base of the Cambrian. We must ask what stratified system, or systems, of rocks occupies this vast time interval, an interval roughly equal to that of the five Palaeozoic systems Cambrian to Carboniferous.

As a group name for the unfossiliferous formations lying uncomformably on the schists and gneisses of assumed Archaean age Sir Thomas Holland proposed the term *Purana* (meaning old or ancient), and this term is in current use in India (*Trans. Min. Geol. Inst. Ind.*, i, p. 47, 1906; and *Mem. G.S.I.*, li, pt. 1, p. 147, 1926). The term Purana corresponds roughly to the Algonkian, comprising the Keweenawan and Animikie systems of North America. To this group Holland assigned the Vindhyans and the underlying Cuddapahs, two great systems of stratified sedimentary rocks, as well as other formations thought to be their equivalents. It is unknown whether the upper part of the Vindhyans extends into the Cambrian, as is not unlikely, or whether the entire system, Upper and Lower, is of pre-Cambrian age. In either case it is shown by Professor Holmes' work that there is ample room in the time scale, in the gap above the Delhis, for both the Vindhyan and the Cuddapah systems.

A point of special interest is that Dr. Heron has suggested that the Delhis are not an Archaean system, but may instead represent a metamorphosed form of a Purana system, namely the Cuddapahs. In the latter case we should be left with only the Vindhyans, minus any portion that happens to be Cambrian in age, to occupy the space of 235 million years. In assessing the likelihood of the Vindhyans alone being sufficient for this gap we must remember that primitive chitinous fossils (F. Chapman, *Rec. G.S.I.*, lxix, p. 109, 1935; M. R. Sahni, ibid., p. 458) have been found in the Suket shales, the topmost stage of the Lower Vindhyans. But we must remember also that the Peninsula of India is accustomed to long spells of relative inactivity as a land mass, and that a break in deposition of say 100 million years would not be out of accord with the later history of the Peninsula.

It is obviously most desirable that some method should be found of assessing in years the age of the Cuddapah system in its original home, namely the Madras Presidency, in order to settle whether it is younger than the Delhis of Rajputana, or is instead the equivalent thereof.[2]

[1] Perhaps a comparison of the variations in the usage of the term "Huronian" would have been apter.

[2] As far as I know there are no pegmatites intrusive into the Cuddapahs, the principal intrusives being basic sills.

In fact to settle the age of the Cuddapahs—whether they are younger than or are the equivalent of the Delhis—is more important to the understanding of the pre-Cambrian stratigraphy of the Peninsula of India than to determine the absolute ages of the Archaean systems below the Delhis. And as the next province on my list for discussion happens to be Province No. 5—Rajputana-Gujarat—I should be extremely grateful to Professor Holmes, or to any other student of radio-active methods, if the answer to this question could be forthcoming quickly.

L. L. Fermor.

24 Durdham Park,
Bristol, 6.

18th January, 1950.

OCEANIC MEANDERINGS

Sir,—During the first world war the slogan used to be—Join the Army and see the World; Join the Air Force and see the Next. In the second war even ordinary citizens were allowed reasonable opportunities for extra tellural exploration. Now we are enjoying a lull during which various Air organizations have perfected the means of conveying a body around the world at high speed with unique opportunities for seeing nothing under conditions of considerable discomfort.

I have recently made such a journey around the world and, excluding my objectives which were a tour in Papua and a visit to Melbourne and New York, the scenic value of my trip was confined to a view from a great height of the Arabian desert, an eight-hour stop at Honolulu, and about two hours of the crossing from San Francisco to New York. For the rest, the solid world was obscured either by cloud, darkness, or oceans. The Pacific and Atlantic crossings were uneventful; the former took two days and was complicated by Saturday, 10th December repeating itself, the latter only five and a half hours—thanks to a 150-mile an hour tail wind at 20,000 feet—but even these were hours of darkness.

These immense oceans have an irresistible fascination and as the aircraft winged its way above the deep blue expanse bending out of sight beyond the curving horizons, one pondered on their significance and past history. I, personally, like so many of us have been through the early enthusiasms for the Wegener hypothesis and have come out at the other side. To my mind there is no acceptable case for the Atlantic and Indian Oceans being other than submerged sectors of a once continuous continental mass though traversed by geosynclines and orogenic systems of various ages with a pattern comparable to that of the present land areas. The Pacific is less simple but, even in this case, if we free our minds from prejudice and take stock of the evidence critically there are many geological pointers to this great ocean being a foundered area of ordinary continental character, perhaps traversed by fold systems and certainly subject to undulations of the ocean bottom on a large scale.

I write this letter not to expound and document my present point of view but to call attention to a considerable body of new evidence which is accumulating on the problems of oceans. The basis of the theory of the permanence of oceans, including the Pacific, is being badly shaken and the time is coming for a new stocktaking with many new facts and changed ideas to work on.

The hypothesis of permanence of oceans is founded partly in very insecure geological speculation and partly on the geophysical deduction that there is an essential difference between continents and oceans, thereby erecting the sial-sima fantasy. Geologists have been reluctant to admit that fold systems, and therefore their flanking continents, can continue into the oceans beyond the last point seen; if Fiji, for example, had not chosen to exhibit itself the

western margin of the Pacific would have been accepted as close to the New Zealand–New Caledonia line. The history of the Rockies geosyncline throughout the Palaeozoic and Mesozoic indicates the former existence of a western continent and continental massifs are still exposed on the western flank of the orogen, the Chiapas massif in Mexico, for example.

The relationship between elevation of mountains and the effects of compression in the production of thrust and folded zones requires much more study than it is normally given. Is the elevation of a compressed zone a necessary sequel? We know that most mountain zones owe their present elevation to a broad up-warp subsequent to the compressive forces but perhaps in some cases the last phase may be of depression, as in certain zones of the East Indies. The great elevation of Greenland, and of the great Hawaian chain rising out of the Pacific, have the physical dimensions of Alpine zones but in neither case have we evidence of compressive forces.

The geophysical case for permanent oceans is based on gravity and seismic observations. It has been thought that gravitationally the oceans are different from the continents in having more uniform values dominantly positive but as more and more data accumulate this simple deduction is found not to hold. G. P. Woollard has recently published an important analysis of gravity values around the world and his conclusion is that " No sweeping statements can be made concerning any universal difference between the crust beneath the oceans and the continents from gravity data alone " (*Trans. Amer. Geoph. Union*, 30, 1949, p. 200). As gravity evidence accumulates more and more holes are found in that convenient basket for geological loose thinking named isostasy and this also may have to go into discard.

The seismic evidence in the past has indicated that the deep oceans lack a crust of continental character but this sweeping generalization may require modification. As samples of recent work I might quote the results obtained by B. C. Browne and his colleagues at one of the Atlantic Ocean Weather Ships (53° 51¹ N., 18° 40¹ W.) where below a depth of water of 1,300 fathoms and 18,000 feet of low and medium velocity layers, presumably sediments, there was found a floor with a velocity of 21,000 ft./sec. This is average continental basement velocity—and could even be massive limestone. Another sample from the Pacific is from Bikini where H. S. Ladd and J. I. Tracey obtained a velocity 17,000 ft./sec. at about 10,000 ft. depth (*Sci. Monthly*, Nov., 1949) which they regard as basement, but if so it is basement of a continental character or alternatively it might still be sediments.

Evidence of the physical relief and composition of the ocean bottoms is also accumulating. S. J. Shand (*Journ. Geol.*, 57, 1949) has examined rock samples from the mid-Atlantic Ridge and found strong evidence (mylonitized anorthositic gabbro) of shearing stresses during or after the formation of the Ridge. The relief of the Atlantic bottom has been found to be as varied as that across the average continent. In the Pacific recent discoveries of " sea mounts " have been described by F. P. Shepard (*Submarine Geology*, 1948). These are plateau topped protuberances from the deep ocean floor at depths below present surface of 500 to 600 fathoms and suggestive of erosion surfaces. The evidence of both submarine canyons and these sea-mounts points to a marked relative subsidence of the ocean bottom in certain areas in recent geological time.

These are somewhat random thoughts, perhaps insufficiently developed, provoked by my journey and by a study of recent literature. I have set them down in order to call attention to the unsatisfying state of many existing geological ideas and to stimulate a more critical approach.

BRITANNIC HOUSE,
LONDON, E.C. 2.
31st January, 1950.

G. M. LEES.

REVIEWS

INTRODUCTION TO COLLEGE GEOLOGY. By CHAUNCEY D. HOLMES. pp. xxi + 429, with 312 text-figures (half-tone and line). New York and London : Macmillan and Co., Ltd. 1949. 30*s*. (pre-devaluation).

HISTORICAL GEOLOGY. By CARL O. DUNBAR. pp. xii + 567, with 17 plates and 350 text-figures (half-tone and line). New York : John Wiley and Sons, Inc. London : Chapman and Hall, Ltd. 1949. Price 30*s*. (pre-devaluation).

OUTLINE OF HISTORICAL GEOLOGY. By A. K. Wells. 2nd Edition (revised with the assistance of J. F. Kirkaldy). pp. xiv + 356, with 2 plates and 124 text-figures (line). London : Thomas Murby and Co. 1948. Price 25*s*.

These three text-books show interesting differences in their approach to a study of historical geology. The contrast between the English and American treatment is to some extent inevitable in view of the size-discrepancy between the canvases upon which the picture is painted—Professors Dunbar and Holmes have to cater as much for Boston, Mass., as for San Diego, Cal. The effect is to enforce a more diffuse and generalized survey than is usual in the corresponding type of European text-book. This is accentuated by the inclusion of separate sections of palaeontological material, much of it purely morphological, and perhaps rather more palaeogeographic reconstructions than the texts are able to support.

Professor Holmes aims at presenting all geology, as it were, in a single dose. The reader emerges with that breathless feeling which is the after-effect of a high-speed conducted tour, and this sensation is accentuated by the Voice which assails him at the end of each chapter : " What are brachiopods ? Define geyser. Name and date the rock formations mentioned in this chapter." Professor Dunbar, on the other hand, pursues a more familiar course, revising and amplifying a series of forerunners—Pirsson and Schuchert, and Schuchert and Dunbar. Here the advanced student will find much stratigraphic and interpretative detail presented generally in a form suitable to serve as a basis for, or summary of, further study. There is nevertheless a feeling of vagueness about some implied correlations, which could easily be removed if outstanding uncertainties could be clearly stated.

In comparison with its American companions, " Wells and Kirkaldy " suffers from the prevalent austerity in publishing. The general arrangement will already be familiar to most British geologists, and as a compact account of British historical geology it will command a high degree of respect. It is perhaps unfortunate, particularly in the earlier chapters, that occasional excursions into world stratigraphic relations could not have been made. The nature of the record often makes the broad view the only one possible. As would no doubt be expected the sections dealing with Cretaceous and Tertiary Britain are extremely well done. A minor criticism concerns a prevalent habit in presenting stratigraphic information, common to many authors in this country : without the assistance of some device fitting vertical sections into the regional picture, such diagrams tend to miss their point as the means of clarifying complex rock-situations. The point of view of the advertising expert would be a useful weapon in the hands of the authors of most stratigraphic works.

T. G. M.

GEOLOGY FOR ENGINEERS. By JOSEPH M. TREFETHAN. xi + 620, with 9 plates and 234 text-figures (half-tone and line). London : Macmillan and Co., Ltd. 1949. Price 29*s*. New York : D. Van Nostrand Company, Inc. 1949. Price $5.75.

The presentation of the geological aspects of engineering practice at the present time normally falls into a standard pattern. One approaches any new work expecting either variations on a well-known theme, or else some entirely new design, perhaps based on original teaching methods. In its

general scope, Dr. Trefethan's book fulfils the first of these expectations, but in its arrangement it satisfies neither. Thus the first ten chapters deal, in the orthodox manner, with minerals, rocks, the geological cycle, structures, and field geology, but contain as well a good deal of technical engineering material. The middle chapters are curiously arranged—earth history geologic maps, the atmosphere, subsurface water, earth movements, earthquakes, streams—and the effects of glacial action are not discussed until the end of the book is almost reached, *after* a section dealing with dam sites and reservoirs.

If the confusing effect of this somewhat illogical sequence can be overcome the more completely engineering sections are seen to be generally sound, and the illustrations are clear and well designed throughout. There is naturally a tendency towards the large in scale when questions of reservoir-silting, soil erosion, and shoreline maintenance are being discussed, and these are certainly the best sections in the book. It is unfortunate that the problems peculiar to the early stages of heavy structure planning could not have been gathered into one chapter; perhaps a later edition may do this.

T. G. M.

PROBLEME DER NATURWISSENSCHAFTEN ERLÄUTERT AM BEGRIFF DER MINERALART. By P. NIGGLI. pp. xi + 240, 99 text-figures. (Sammlung "Wissenschaft und Kultur", Band 5.) Verlag Birkhäuser, Basel, 1949. Price 18.50 Swiss francs.

Professor Niggli's recent publications have accustomed readers to seek in his writings a display of wide erudition and broad synthesis. This small volume is no disappointment. A philosophical introduction on methods of research in natural science is followed by a discussion of the crystal as individual—crystal symmetry and structure types, ideal crystals, and real crystals (mosaic structure, "crystal pathology"). The study of crystal morphology is here placed in proper perspective; it is "by no means merely description and illustration; he who believes this has completely failed to grasp the essence of the method". Interesting analogies are frequently drawn. Crystals can be formally grouped in hypocubic and hypohexagonal lineages, but such groupings must not be given a genetic interpretation, any more than the palaeontologist should, without further evidence, rashly accept a morphological series as necessarily indicating a line of descent. Consideration of the basic principles underlying the grouping of individuals into species leads to a discussion of variation from the ideal, whether as internal variation due to substitution and mix-crystal formation or as a consequence of external conditions (polymorphism, plastic deformation).

The book, which appears as one of a series intended for non-specialists, is designed to appeal especially to those, such as biologists and chemists, who are closely concerned with the concept of species. Some of the matter on symmetry and space-groups is not easy reading. Interested readers of this review, however, would bring to bear some knowledge of crystallography and mineralogy; they can scarcely fail to be stimulated by the author's discussion of established facts viewed from a new angle.

F. C. P.

ORE GENESIS: A METALLURGICAL INTERPRETATION. By JOHN STAFFORD BROWN. pp. 204, with 4 figs. Thomas Murby and Co. London, 1950. Price 12*s.* 6*d.*

The genesis of ore deposits has long been one of the most controversial problems of geology and a great variety of hypotheses have been put forward, some remaining in fashion longer than others. The prevailing theory of a dilute, hydrothermal ore-fluid of magmatic origin, though controversial in detail, enjoys a large measure of acceptance in principle, but even its most fervent disciples often find it exasperatingly difficult to account for the observed features of mineral paragenesis and zoning. It is not surprising,

therefore, that from time to time a student should become dissatisfied with the current view and evolve or revive in modified form an alternative theory. Such is the case with Dr. Brown who, seeking inspiration from the blast furnace, puts forward a metallurgical interpretation of ore genesis.

Dr. Brown was diverted from the hydrothermal faith as a result of his geological studies in the base-metal field of Balmat, New York, where he considered that only a Fe-Zn-Pb-S vapour could have transported and deposited the elements and minerals involved. The present book embodies the result of a subsequent survey of facts of mineral occurrence viewed in the light of selected chemical data and metallurgical experience.

The author envisages the early cooling history of an originally molten earth, resulting in the gravity separation of immiscible liquids arranged in depth in order of increasing density. It is suggested that pegmatite magma, with tin and tungsten, originates toward the middle of a light acid silicate (granitic) layer, which rests at a depth of 25 km. on a more basic silicate (basaltic) layer. Iron source-magmas with oxides and silicates separate about the middle of this basic stratum. Below these, at a depth of 60 km., there is a heavy sulphide layer, averaging 1,250 feet in thickness, which was prevented from sinking deeper by the early crystallization of a peridotite " floor ". On the basis of metallurgical experience, it is assumed that the sulphide layer is subdivided from top to bottom into (i) an iron-copper sulphide matte, probably containing appreciable zinc sulphide ; (ii) an iron-antimony-arsenic speiss with some copper and considerable lead, together with any nickel and cobalt that may be present ; and possibly (iii) a lead sulphide layer with considerable copper sulphide and antimony, accompanied by bismuth and most of the gold and silver. These layers, while not necessarily continuous, have essentially a world-wide distribution.

The constituents of these source magmas may be volatilized and migrate upwards as simple mineral vapours wherever the surrounding material becomes crystalline and susceptible to fracturing. Since crystallization of the host silicate layers should take place from the surface downwards, the lightest and uppermost source-magma (pegmatite) will be " tapped " first, followed by the successively deeper and denser iron source-magmas, and finally the sulphide layers. In this way the author accounts for the normal " order of arrival " of the ore minerals.

It is contended that relative volatility governs the distances to which the vaporized mineral constituents may travel, thus accounting for mineral zoning. The role of magmatic water in transporting the ore-forming elements is discounted as negligible, but meteoric waters are thought to be of great importance in promoting ore-deposition when encountered by the mineral vapours. The aid of such vadose waters is invoked for removing rock material liberated by replacement and for the formation of gangue minerals.

The observed field relationship between igneous rocks and mineralization is a result of combined structural control and the local rise of geo-isotherms, caused by the emplacement of hot rock magmas.

In developing this hypothesis of ore genesis and applying it to several varied examples of mineralization described in the literature, the author has made a contribution which should stimulate discussion and no little criticism. In the reviewer's opinion, the hypothesis might well have been supported by a wider consideration of the fundamental geological aspects, including geosynclinal geology and the granite problem, not to mention considerations of diffusion and other physico-chemical factors, and the structure of evidence supporting the hydrothermal theory. The author's metallurgical foundation may be strong enough ; his geological foundation will give his critics many opportunities.

As long ago as 1839 de la Beche wrote : " It has often been observed that there have been few subjects more fertile in theory than that of the formation of mineral veins, and it may be added that there have also been very few, the consideration of which have in general been productive of less satisfactory results "—the situation has changed but little in the meantime.

J. S. W.

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